SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, APRIL 7, 1905.

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THE MUTATION THEORY OF ORGANIC EVOLUTION.*

The Mutation Theory of Organic Evolution, from the Standpoint of Animal Breeding: W. E. CASTLE, Assistant Professor of Zoology, Harvard University.

The mutation theory, as I understand it, is not designed to replace Darwin's theory of natural selection, nor is it capable of replacing that theory. Natural selection must still be invoked to choose between different organic forms, preserving the more efficient, destroying the less efficient. The question raised by this new theory is, What sort of forms are subjected to the action of natural selection? Is there a complete gradation of forms between two extreme conditions and is natural selection called upon to choose from this whole series the one which is organically most efficient, or is the task simpler and is the choice made merely between two widely separated conditions of the ideal series? Thus, we find within a species two varieties, one larger than the other. Have they diverged by gradual cumulation of minute differences in size, or by a single step? These alternative views are known, respectively, as the selection theory and the mutation theory. Both views were recognized by Darwin as possibilities, though he seems to have attached more importance to the process of gradual modification. Most of his followers have given attention exclusively to this process, but a few, like Bateson and de Vries, have regarded modification by

* Six addresses given before the American Society of Naturalists at Philadelphia, December 28, 1905.

steps as the more important process, if not the only one efficient in the formation of new species. Bateson ('94) has called modifications of this sort discontinuous variations, but de Vries (:01–:03) calls them mutations, and the latter designation seems likely to be generally adopted.

Darwin rightly attached great importance to the variations of domesticated animals and plants as throwing light on the origin of species. He recognized that there is no essential difference between breeds and species, and that if we can ascertain how breeds originate we can infer much as to the origin of species. He made an extensive study of breeds of animals as well as of plants, but no one has followed this up or even recognized its great importance until within very recent years. What we need to know is how, precisely, are new breeds formed. We know that they are forming under our very eyes all the time and that this has been going on since the earliest historic times and no doubt a great deal longer, yet the method eludes us.

The successful practical breeder, the man who originates breeds, is a keen observer, a man of unusual intelligence and skill and of infinite patience. Yet if we ask him how, in general, he does his work, or how a particular result was obtained, we rarely get a satisfactory answer. This is sometimes because, for commercial reasons, it is well to leave a cloud of obscurity surrounding the origin of a successful breed, lest its production be duplicated. More often, however, it is because the breeder himself does not know how the result was attained. He may be able to tell us that such and such animals were mated, such and such of their offspring selected, and after a certain length of time the breed was established and put on the market. But this, after all, gives us little information as to the real nature of the material used and the processes involved in the formation of the new breed. The aims of the biologist are so different from those of the practical breeder that to solve the theoretical problems involved in the formation of breeds the biologist must himself turn breeder, and see new organic forms arise out of material with which he is thoroughly familiar, and under conditions which he can control. So little work of this kind has yet been done that its fruits are scarcely ready to be gathered. Generalizations can as yet be made only tentatively, based on cases dangerously few, or on the rather uncertain and often contradictory testimony of practical breeders and the half-truths told by stock registers.

So far, however, as these various sorts of evidence go, they indicate that the material used by breeders for the formation of new breeds consists almost exclusively of mutations. The breeder does not set to work with some purely imaginary form in mind, toward which he seeks by selection gradually to mold his material. He commonly either *discovers* the new breed already created and represented by one or more exceptional individuals among his flock, or else he seeks by cross-breeding to combine in a single race characters which he finds already existing separately in different races. In both cases he deals with mutations, *i. e.*, with characters unconnected by a series of transition stages with the normal form. An illustration from my own experience may help to make this clear. A little more than four years ago I obtained a number of ordinary smooth-coated guinea-pigs and began breeding them with a particular experiment in mind. Among nine young produced by a certain pair, there was one which had a supernumerary fourth digit on one of its hind feet. Neither of the parents had such a digit, nor had I ever heard of the existence of such a character before, either in any of the wild Caviidæ, or among domesticated cavies or guinea-

pigs. Further, I have been able to find no reference to such a thing in the literature of the group, though I have several times since found this same mutation in other herds of guinea-pigs. The mother of my four-toed pig never produced another similar individual, though she was the mother in all of thirty young. The father, however, who sired in all 139 young, had five other young with extra toes, but these were all by females descended from himself, so that it seems certain that the mutation had its origin in this particular male. By breeding together the four-toed young and selecting only the best of their offspring I was able within three generations to establish a race with a well developed fourth toe on either hind foot. This race was not *created* by selection, though it was *improved* by that means. Like the poet, in the proverb, it was *born*, not made. Any amount of selection practised on other families of my guinea-pigs would probably never produce a four-toed race, for though carefully watched through as many as seven generations no four-toed pig has appeared among them.

In a second family of my guinea-pigs, which, like the other, was for the purpose of a particular experiment inbred, a different mutation made its appearance. A few individuals were found to have hair about twice as long as that of their parents and grandparents. Intermediate conditions did not occur. Long-haired individuals mated together were found to produce only long-haired young, so that a new breed was already fully established without the exercise of any selection. It was found, in short, that the long-haired character is a Mendelian recessive in relation to the normal short coat, so that matings between long-haired and short-haired animals produce only short-haired young. But these young bred *inter se* produce a definite proportion (about one fourth) of long-haired young, and if no selection is practised among their offspring, but all are allowed to breed freely, the race will continue to contain this proportion of long-haired individuals.

If such a mutation as this occurred in a state of nature, and such a possibility we can scarcely question, a dimorphic species would be the immediate result, containing two varieties alike in every respect except length of hair, in which they would be sharply separated. The two varieties would coexist in the same habitat and might continue to interbreed freely without the destruction or necessary modification of either. Natural selection would now come into operation to choose between the two that one which was more advantageous and the other condition would be gradually eliminated from the race. Or if the two conditions were each the better in a different habitat, then by the gradual destruction of the other in that habitat the two varieties would become geographically separated, though they might continue to coexist in an intermediate zone.

The second method which I mentioned for the artificial production of new breeds is to combine in one race characters already found in different races. This is accomplished through cross-breeding and is made possible by the facts (1) that mutations are alternative in heredity to the normal condition, and (2) that one mutation is entirely independent of another in heredity. If, for example, we cross long-haired with short-haired guinea-pigs, we get, among the second-generation offspring, a mixture of long-haired and of short-haired animals, but, as a rule, no intermediates. Further, if in the original cross one parent was four-toed and the other three-toed, then in the second generation offspring we get all possible combinations of the characters involved in the cross, viz., long-haired four-toed animals, long-haired three-toed, short-haired four-toed and

short-haired three-toed. From this array of forms the breeder may now select the particular combination of characters which suits his purpose.

Can we doubt that in nature a similar choice is offered between every mutation and its opposite, combined or uncombined with every other mutation then present in the race?

It is true that cross-breeding may affect to a greater or less extent the nature of the characters involved in a cross, but this sometimes facilitates the creation of desirable breeds, for it serves to induce new mutation, which in some cases is progressive, in others regressive. For example, in guinea-pigs, a cross between a coal-black animal and an albino may restore in the young the ancestral, or 'agouti,' coat consisting of black hairs ticked with reddish yellow, or in other cases may result in the production of a black-white spotted animal. By selection either of these conditions may be perpetuated in a distinct breed. The one is a regressive or reversionary change, the other progressive in that it leads to the production of a new type of pigmented coat.

On the whole, it appears that the formation of new breeds begins with the discovery of an exceptional individual, or with the production of such an individual by means of cross-breeding. Such exceptional individuals are mutations.

An examination of stock registers points in the same direction. The beginnings of new breeds are small. Pedigrees lead back to a few remarkable individuals or to a single one, as in the Ancon sheep. But given the exceptional individual, and a new breed is as good as formed. The few generations which the breeder usually employs in 'fixing' or establishing the breed and during which he practises close breeding serve principally to free the stock from undesirable alternative characters, not to modify the characters retained.

Modification of characters by selection, when sharply alternative conditions (*i. e.*, mutations) are *not* present in the stock, is an exceedingly difficult and slow process, and its results of questionable permanency. Even in so-called 'improved' breeds, which are supposed to have been produced by this process, it is more probable that the result obtained represents the summation of a series of mutations rather than of a series of ordinary fluctuating variations. For mutations are permanent; variations transitory. A moment's reflection will indicate the probable reason. Variations which are distributed symmetrically about a modal condition, so as to produce when graphically expressed a frequency of error curve, represent the result of a number of causes acting independently of each other. These causes are principally external, consisting in varying conditions of food-supply, temperature, density, moisture, light, etc. These conditions alter from generation to generation, and so do effects dependent upon them. Mutations, on the other hand, have an internal origin, in the hereditary substance itself. They are relatively independent of the environment, being affected only by such causes as affect the nature of the hereditary substance itself, one of which apparently is cross-breeding.

There are, however, frequently found in breeds of domesticated animals conditions which are *not* sharply alternative in heredity to the corresponding characters of other breeds. It is an open question whether such conditions could be maintained if cross-breeding were freely allowed with animals of a different character. If not, they could scarcely become racial characters, under the action of natural selection. The race would then become, not sharply dimorphic or polymorphic, as is

the case where inheritance is sharply alternative, but subject to extremely great fluctuating variations. It is open to question whether blending characters of this sort found in many breeds may not have been created by selection from masses of fluctuating variations. It will be important to know further whether or not these extreme fluctuating series have had their origin in mutations. Not improbably, as de Vries has in part suggested, one-sided variation curves indicate the occurrence of mutations of this sort.

The Mutation Theory From the Standpoint of Cytology: EDWIN G. CONKLIN, professor of zoology, University of Pennsylvania.

I. The mutation theory is founded upon the idea that mutations are primarily germinal, that they arise in one or both of the sex cells and only later appear in the adult organism. In contradistinction to certain theories of evolution which are concerned chiefly with the modifications of adult structures, the mutation theory is primarily concerned with modifications of the germ, and here it comes into direct relation with the science of cytology.

De Vries tells us that the foundations of the mutation theory were laid in his doctrine of intracellular pangenesis. Like Darwin, Galton, Weismann and many others, he recognized the fact that the method of evolution is at bottom a problem of inheritance and that, in the words of Osborn, 'When we have reached a heredity theory that will explain the phenomena of inheritance, the method of evolution will itself be a thing of the past.'

It seems like a mere truism to affirm that the evolution of animals and plants must be accompanied by an evolution of their germ cells, and that the principal problem of evolution is not how modifications are produced in adults, but how they arise in the germ. And yet with few exceptions previous theories of evolution have concerned themselves only with the transmutations of adult forms and have paid no attention to the modifications of the egg or sperm or embryo. The mutation theory is a theory of the evolution of organisms through the evolution of their germ cells and it is, therefore, founded primarily upon cytological phenomena.

An antecedent objection to any such theory is the very general opinion that the germ cells are composed of 'simple, undifferentiated protoplasm' and that they do not contain specific morphological elements upon which evolutionary forces might act. However, such a view is supported neither by observation nor by the latest and most careful experiments. We know that the cell is vastly more complex than was assumed a few years ago, and there is no good reason for supposing that all of its visible structures are now known. The fact that fragments of eggs may in some instances give rise to entire embryos does not necessarily imply, as is usually assumed, that the egg is undifferentiated. In eggs, as in adult forms, the degree of differentiation may be largely independent of the power of regeneration or regulation, and certainly such experiments do not nullify the most positive and direct evidence, drawn from many sources, as to the complexity of the germ.

Extensive studies which have been made upon the structure of the nucleus have brought to light a degree of organization in this part of the cell which was wholly unexpected. It has long been known that in any given species the number of chromatic threads or chromosomes in the nucleus is constantly the same in all kinds of cells, except in the last stages of the formation of the sex cells, where the number is one half the normal; in the union of the egg and sperm nuclei in the fertiliza-

tion the normal number is again restored. Van Beneden, Boveri, Hertwig and others have shown in the most convincing manner that these chromosomes are the principal seat of the material substance concerned in hereditary transmission, and recently Boveri has determined that the several chromosomes are individually different in the heritable qualities which they bear. Coincidently with this notable discovery Montgomery found that the reduction of the number of chromosomes in the sex cells is effected by the union of chromosomes into pairs and he showed good reason for believing that one member of each pair came from the father and the other from the mother. This conclusion was confirmed and extended by Sutton, and he, as well as others, pointed out the suggestive fact that in the maturation divisions immediately preceding the formation of the ripe egg or sperm only one chromosome of each pair goes into each mature germ cell. This parental purity of individual chromosomes in the sex cells corresponds to the purity of parental characters in the experiments of Mendel and the chance combination of these chromosomes into pairs in fertilization corresponds with the combinations of qualities (the Mendelian ratio) in alternate inheritance. These remarkable discoveries as to the organization of the nucleus demonstrate that the germ cells are by no means simple and undifferentiated, as has often been affirmed, but rather that they contain numerous visible morphological elements, each of which has a particular rôle in hereditary transmission, and they suggest that modifications of these elements, however produced, are the real causes of evolution.

Much work has also been done upon the differentiations of the cytoplasm in the egg cell (obviously the sperm is unsuitable for such a study); with few exceptions the earlier experimental work led to the conclusion that the cell substance was isotropic and undifferentiated. However, many careful observations have shown that in the case of many animals the cytoplasm is visibly differentiated in certain areas of the egg and that the substances of these areas give rise in the course of development to particular organs or parts of the embryo. It is now known that in the eggs of a considerable number of animals belonging to several different phyla all the axes and planes of symmetry of the future individual are marked out in the unsegmented egg, and in the case of certain annelids, mollusks, echinoderms and ascidians it has been discovered that the substances of the ectoderm, the mesoderm and the endoderm are visibly differentiated in the egg before cleavage begins. In certain ascidians I have found that all the principal organs of the larva, viz., the muscles and mesenchyme, the gastric endoderm and general ectoderm, the nervous system and notochord, are all represented in the two-cell stage by visibly distinct substances which are definitely localized in the egg. These facts show that in certain groups of animals there is such a thing as a morphology—not merely a promorphology—of the ovum and they demonstrate that there are morphological elements in the cytoplasm upon which evolutionary forces may act.

It is a matter of prime importance to know whether the nucleus contains the only hereditary material carried over from one generation to another or whether certain characters, such as polarity, symmetry and the localization of organ bases in the egg, may not have their seat in the cytoplasm. It is as yet too soon to make any positive assertions on this point but the evidence seems to favor the view that the nucleus is at least the principal seat of the inheritance material. Even in cases where the cytoplasm of the egg is so highly differentiated as in ascidians this conclusion

probably holds good. The three principal kinds of protoplasm of the ascidian egg before cleavage are the ectoplasm which gives rise to ectoderm, the mesoplasm which produces mesoderm and the endoplasm which becomes endoderm. Each of these three substances is derived in part from the nucleus of the egg; the ectoplasm comes from the egg nucleus at the beginning of the first maturation division, the other two from the nucleus at an earlier stage in the ovogenesis. In every cycle of division a large amount of chromatic material escapes from the nucleus into the cytoplasm and I have found in mollusks and ascidians that this substance is then differentially distributed to different areas of the egg and that it gives rise in part to the principal formative substances of the embryo. These facts lend support to the hypothesis of intracellular pangenesis proposed by de Vries; they show that even though a certain number of general differentiations may be transmitted through the cytoplasm, such as polarity, symmetry and localization, nevertheless the mechanism exists for the nuclear control of the cell and they thus afford a means for harmonizing the facts of cytoplasmic organization with the nuclear inheritance theory.

We find, therefore, that the germ is by no means simple, even if we consider only the visible structures of the cell, and that its organization is sufficiently complex to exercise a determining influence upon development and evolution. Similarities in the character and localization of the material substances of the egg must be the initial causes of all similarities or homologies which appear in the course of development. Modifications of this germinal organization, however produced, are probably the immediate causes of evolution.

II. If we inquire how such modifications of the germ arise and what the particular modification is which is associated with a certain mutation of the adult organism, we pass from the region of observed fact to one of hypothesis, for in only a few instances have such germinal mutations been observed. Nevertheless, enough is known regarding the organization of the germ cells to warrant our hazarding a 'shrewd guess' as to the nature of these germinal mutations. The nuclear inheritance theory points to some modification in the structure, number or distribution of the chromosomes or of the elements of which the chromosomes are composed as the initial cause of mutation. The fact that the Mendelian ratio in alternate inheritance corresponds to the ratio of chromosomal distribution in the maturation and fertilization of the egg indicates that in such chance distribution of these chromosomes we have the principal cause of the law of alternate inheritance, as Wilson, Sutton, Cannon and others have pointed out. It may also be reasonably inferred that in this chance distribution of chromosomes we have one of the most potent causes of individual variations.

It is well known that the disappearance of characters does not necessarily imply their final loss; many heritable qualities remain latent through one or more generations, only to appear as active characters in subsequent generations. In such cases it is probable that the material bearers of these qualities also remain latent, though we are wholly ignorant of what constitutes latency as contrasted with activity in chromosomes.

Relatively little is known as to the factors which determine the number of chromosomes or as to the effect of varying numbers on adult organization. Montgomery concludes from his studies on the Hemiptera that certain chromosomes are in the process of degeneration and disappearance in these animals. In this case the heritable qualities which were borne by these chro-

mosomes would also disappear, thus constituting a case of 'regressive mutation' in the terminology of de Vries. In other cases there has evidently been an increase of chromosomes in some species as compared with others; though whether this increase is due to the division of originally single chromosomes, or to the addition of new ones through abnormalities of division or distribution, or through hybridization, can not now be determined. Irregularities in the number and distribution of chromosomes are by no means uncommon and in the case of hybrids are very frequent, as has been shown by Juel, Guyer and Cannon. By an increase in the number of chromosomes or in the number of elements of which a chromosome is composed the sum of the heritable qualities would probably be increased, thus constituting, in the language of de Vries, a 'progressive mutation.' There is no evidence and no probability that new chromatic elements are ever added to the nucleus from the cytoplasm, or that they ever arise *de novo*. Such new elements must arise through new combinations of old elements, either, as de Vries considers, by an actual interchange of Anlagen (material particles) between the pairs of maternal and paternal chromosomes, or, as Haecker supposes, by an interchange of grandparental parts of chromosomes, or through hybridization or irregular mitoses. Guyer has shown that the divisions of the chromosomes are frequently or usually irregular in hybrids and he suggests that such irregular mitoses may add or subtract certain chromosomal elements and thus constitute the basis for a mutation. Such cases are, however, almost entirely hypothetical and at present we are compelled to admit that we do not know how mutations arise or first become manifest in the chromatin.

As regards the cytoplasm, I have shown reason for believing that it is composed in part of escaped nuclear material and that the mechanism, therefore, exists for the nuclear control of the entire cell. The organization of the cytoplasm is chiefly manifest in its polarity, symmetry and the localization of unlike substances. There are reasons for believing that many bilateral animals are characterized by fundamental similarities in the polarity and symmetry of the unsegmented egg; the types of localization of organ bases are, however, very different in different phyla; in particular the localizations in the eggs of ctenophores, nemerteans, echinoderms, annelids, mollusks and ascidians are thoroughly characteristic of each phylum, and except in the case of the annelids and mollusks there are few similarities between these types. Nevertheless, it is possible that certain of these types may have been derived from others; in fact, such transformations might be accomplished far more easily in the egg than in the adult.

Despite the evident and almost insuperable difficulties involved, certain zoologists have not hesitated to indicate how the adult form of one phylum might have been derived from the mature form of another; thus we have the cœlenterate, the nemertean, the echinoderm, the annelid and the arthropod hypotheses as to the origin of the vertebrates, and in each of these cases by stupendous transformations, degenerations and new formations of the adult form of the invertebrate in question the vertebrate is supposed to have sprung into existence fully formed and panoplied, like Minerva from the brain of Jove. In all these speculations fancy occupies so prominent a place and facts are so scarce that it is no wonder that the whole 'phylogeny business' has come into disrepute. Nevertheless, the evolution idea compels us to assume that there are relations more or less remote between all phyla and that some must have come from others by natural processes.

Without attempting to defend any of the hypotheses mentioned it may here be pointed out that relatively slight modifications in germinal organization would convert one type into another.

A distinguishing characteristic of the mutation theory is the recognition of elementary characters or properties which manifest themselves in many separate parts of the adult, as, *e. g.*, the presence or absence of hairs or certain colors; if mutations are germinal the widespread distribution of such characters in the adult are easily explained. Relatively slight modifications of the germ, however produced, may lead to profound and widespread modifications of the embryo and adult. I have elsewhere shown reason for believing that the cause of inverse symmetry which occurs regularly among some species and occasionally among all, man included, is to be found in the inverse organization of the egg, and that this inverse organization may be due to the maturation of the egg at opposite poles in dextral and sinistral forms. This case shows that one of the most remarkable and far-reaching forms of variation with which we are acquainted is the result of relatively slight alterations in the localization of germinal substances in the unsegmented egg.

One of the principal difficulties in explaining the origin, on evolutionary grounds, of different phyla has been the dissimilar locations of corresponding organs or parts. These difficulties are well illustrated by the theories which attempt to derive the vertebrates from the annelids, or from any other invertebrate type. If evolution takes place through transformations of the germ rather than of the adult, it is no more difficult to explain the different locations of corresponding parts in these phyla than their different qualities. Changes in the relative positions of parts which would be absolutely impossible in the adult, may be readily accomplished in the unsegmented egg, as is shown by cases of inverse symmetry. The question is here raised whether some similar sudden alteration of germinal organization may not lie at the basis of the origin of new types.

Mutations: THOMAS DWIGHT, Parkman Professor of Anatomy, Harvard Medical School.

It has been clear from the beginning that evolution, if it be a power at all, must work either by minute modifications or by more or less sudden changes. Darwinism is essentially the doctrine of minute modifications increased by selection and controlled by the survival of the fittest. Darwin insisted most strongly on the importance of minute modifications. While holding that 'strongly marked variations' might modify a species without the help of any selection at all, he absolutely denied any sudden changes of importance such as lie at the bottom of the mutation theory. 'Natural selection,' he wrote, 'acts only by the preservation and accumulation of small inherited modifications'; and he asserted that it would 'banish the belief of the continued creation of new organic beings or of any great and sudden modifications of their structure.'

The mutation theory of sudden jumps and, it may be, of long jumps, is far from new; but it is de Vries's merit to be able to show by demonstration what before was only theory. His hypothetical 'pangens' by which the changes are said to be brought about need not be discussed here. A radical difference between the two theories is this: Darwinism pure and simple is essentially fortuitous; it aims in no particular direction, there is no goal; while mutation by producing suddenly a new species, or at least a subspecies, implies the existence of a type and of a law which under certain conditions becomes operative.

In this discussion the anatomist is at a disadvantage to the botanist and the breeder who can experiment. His argument must be largely analogical. He must consider how anatomical observations, both on races and on individuals, are better explained by one theory or the other. Man's body, for I am speaking as an anatomist and leaving aside all consideration of psychology, man's body, as we find it to-day, does not mutate, but varies. What do the variations tell us? Race anatomy offers many instances of fusion of different races and occasionally suggests the occurrence in the past of a race differing sharply from those around it. Perhaps the best example of a race without approximate relations and most indicative of mutation is that of the pigmies. On the whole, race anatomy tells us little. What we call race characters occasionally appear sporadically where one would not expect them. Thus the Mongolian spot on the sacral region of infants has been recently observed on a child in Bavaria. More or less striking features of the disputed Neanderthal race occur among us. The study of anatomical variations in the dissecting rooms of different parts of the world shows that while in all probability there are different tendencies in different races, the variations themselves are of no practical importance. Thus the palmaris longus is absent in 12.7 per cent. at St. Petersburg and in 40.4 per cent. at Strassburg. The 'candelabra' method of division of the carotid artery occurs in 20 per cent. at Strassburg and in 60 per cent. at Breslau. The average absence of the pyramidalis is 12.7 per cent. at Strassburg, 21 per cent. in Massachusetts, while among the Japanese this muscle is wanting in only 3.5 per cent. I have found the psoas minor absent in 60.5 per cent. against 48.7 per cent. at St. Petersburg. The sternalis, so exceptional in Caucasians, occurring at most in from 3 to 4 per cent. was found in nearly 9 per cent. of the Japanese and certain observations on the living seem to show that a larger series would produce even a much larger proportion. This, together with the rare absence of the pyramidalis in the Japanese, points toward specialization in muscle, a feature which strikes one as in keeping with the characteristics of that race. We are familiar with the fact that there seems to be a certain similarity of character among themselves in the fauna of isolated countries, yet it surprises us to find it manifested in the deeper structures. Hrdlicka has observed a form of human tibia, suggesting that of the gorilla, in over 10 per cent. of African bones, which is almost unknown among the whites and not found at all among the Indians. Yet no one seriously believes that the negroes have any special relationship with the gorillas. This phenomenon of similarity, therefore, implies some agency beside selection.

Leaving race anatomy let us see whether the variations, which we continually observe in the dissecting room, point either one way or the other in this discussion. The theory of change by minute variations receives no support from anatomical observations. Precisely what many thought an illustration of Darwinism is its refutation. Huxley foresaw this when he doubted whether variations might not prove a two-edged sword. The fundamental error into which supporters of evolution by selection are logically driven is the unwarranted assumption that similarity of structure can be explained only by descent. Though not formally stated, this is tacitly accepted almost as an axiom.

The student of variations is oppressed by their multiplicity. Those of the biceps, for instance, are bewildering, presenting forms normal in many orders of mammals and which refuse absolutely to be forced into any line of descent. Some, indeed, are mutually contradictory. It is no more

than a truism to say that if an anomaly is to be explained by reversion the structure in question must have been normal in some ancestor; but in view of the vast number of anomalies, we are forced to believe that this ancestor must have been a museum of anatomical curios of the most diverse natures. It is overlooked that if the explanation of a reversion be true it must apply not to one only, but to every possible deviation of structure that is not pathological. Phylogeny must show nothing in the history of the supracondyloid process, for instance, which will not accord with that of the paroccipital process, or with that of the third trochanter, or with that of each and all of the hundreds of variations which the human body may present. Not only has this accord not been shown, but obvious contradictions have been neglected. An explanation has been sought by referring certain peculiarities very far back: even to a hypothetical common vertebrate stem antedating the classes. We admire the learning and the research; but does the explanation explain?

One of the great difficulties of selection has been to account for the appearance of strikingly similar adaptations or arrangements in species from entirely different lines of descent. Analogous to this is the similar irregular appearance of variations. The fossa prænasalis is a deep, sharply marked depression just below the nasal opening, occurring chiefly in low races. It is not to be confounded with the gradual passage of nose into face which is the rule among mammals. I am not aware that it is found among mammals except in the seal, and even there it is less well defined than it may be in man. Here, then, is a sudden change not atavistic and certainly not progressive. The pronator quadratus in man very rarely sends a prolongation to a carpal bone. I have found this as a variation in a chimpanzee, and Macalister in a lion, but in no mammal is it normal. To find it also in turtles and in the *Cryptobranchus japonicus* does not help us much towards an explanation.

It is very suggestive that in certain variations of the platysma by which it enters into various combinations with the facial muscles, in some cases its fibers are in direct continuity with those of muscles which comparative anatomy teaches belong to another layer. It seems as if nature were striving for a certain effect and is absolutely indifferent by just what means it is accomplished.

One of the most significant points of the mutation theory is that it rehabilitates species with its old-time dignity. Though we flounder in our definitions of species, we can not get rid of the thought that it is something, after all. By a strange paradox it is precisely through variations that the tendency towards stability of species is emphasized. In my observations on the human spine I have found that very frequently the effects of a variation in one part are felt in remote parts and, indeed, throughout the spine. Some of these seem directly teleological, others tend to preserve the type. Thus if the last ribs are very small, and this holds good whether they be the normal twelfth pair or the abnormal thirteenth, the rib before the last is usually exceptionally long. In the case of cervical ribs it is common to find the last rib very small, as if the whole thorax had moved up. In cases where there are only eleven thoracic vertebræ not rarely an increase of their size tends to preserve the proper proportions of the thorax. In the lumbar region there are certain striking characteristics in the spread and in the structure of the last three transverse processes which give a definite shape to the whole region. In many cases of numerical variation there is an evident effort to reestablish normal

features as nearly as the modification will permit.

It is to my mind impossible to find any support for a theory of evolution by minute changes from the study of anatomical variations. I should not venture to say, on the other hand, that they give any direct support to the theory of mutation; but at least they are not in disaccord with it.

Systematic Work and Evolution: L. H. Bailey, Director of the College of Agriculture, Cornell University.

Every object of which we take cognizance must be named if we are to record and convey the ideas associated with it. As the names accumulate, it is necessary that we group them, or provide some scheme of arrangement. We classify all categories, even though we do no more than to arrange them alphabetically. Nomenclature and classification are primary intellectual processes.

The number of organisms that we know has come to be legion. These organisms are described in books. The first descriptions accepted the organisms as they are, without serious inquiry of their origins. Definite names have come to be attached to each kind of organism and definite customs have arisen to control the bestowal of the names. Biological nomenclature has become a rigid bibliographical method.

The first object of classification was to afford a perspicuous arrangement of facts. The facts must be pigeon-holed, else they may be lost. Gradually, however, the idea of relationship between the objects has developed, and these ideas have expressed themselves in crystallized schemes of classification. That is to say, classification of organisms is a combination and compromise of bibliographical method and expression of relationships.

Presently, the organisms themselves began to be studied from the physiological side. It was discovered that at least some of the named groups of organisms are not entities. There are all grades of differences, from those peculiar to one individual to those peculiar to many individuals, and to groups of individuals. The organisms are multifarious and elastic, but nomenclatorial and taxonomic systems are editorial and arbitrary.

We are all now committed to the evolution philosophy as a working hypothesis. The greatest problems in the study of organic nature are the determining of the lines of ascent and the means by which they have come about. We study plastic material; at the same time we are making a desperate effort, at least on the botanical side, towards rigidity of nomenclature. Our ideas of what constitutes species and varieties are free and extensible enough, but our methods of designating these ideas still follow the formalism of a century ago —are in fact more inflexible than they were in the time of Linnæus. If nomenclature is inelastic, schemes of classification within the genus or species must likewise be inelastic, for the classification is but an expression of our ideas of the relationships of the objects that we name. Our nomenclature does not express either the knowledge or the point of view of our time.

The Present Status of Systematic Work. —There are three elements in the discussion of systematic work as related to transmutation theories: (1) The idea of a species, (2) the methods of naming and recording, (3) the classificatory schemes themselves.

It would be profitless at this time to enter into a disquisition as to what a species is. The many discussions of this subject are so many admissions that no one knows. The only point I care now to make is that we all recognize the fact that the single word 'species' covers groups of widely different grades of value, of differentiation,

and of evolutional development. This fact has been brought forcibly to our attention again by the stimulating work of de Vries. There are collective species, elementary species and other grades. Our formal nomenclature in practise recognizes only two grades—'species' and 'variety,' with no two persons agreeing which is one or the other. If there are such differing grades in nature, then we must accept the fact and adopt new technical words for the various grades. This has not been done, at least not in practise, because we have not yet sufficiently clear ideas to enable us to do so. These varying grades of species and varieties are the results of processes of evolution, and some, if not all, of these processes are still in operation. Therefore, the new definitions of species-concepts must rest on physiological or functional grounds, not merely on morphological and anatomical grounds.

Many of us feel that the present methods of nomenclature and description will be outgrown, for these methods are made for the herbarium and the museum, rather than for the field. It is a most suggestive commentary that the botanist may know the 'species' when it is glued on an herbarium sheet, but may not know it when growing. The nurseryman or gardener may know it when growing, but not when it is in a herbarium. This is not merely because the botanist is unfamiliar with the field, or the gardener unfamiliar with the herbarium; these men have different fundamental conceptions of what a species is; they use different 'marks,' one morphological, the other largely physiological. I believe that the gardener is nearer the truth. I recall a characteristic remark made by my master, Sereno Watson, when, in the confidence of youth, I asked whether a certain binomial would be accepted a hundred years from now. He shrugged his shoulders and said quietly, 'I don't know; they may call plants by numbers then.'

I have no intention of proposing any new plan of nomenclature—that would only amuse you. I merely feel, as you do, that a change is imminent. Perhaps we shall hold to our main species-groups for history's sake, and then designate minor groups in terms of their qualities. If we find it to be true that there are fluctuating varieties and mutations of differing geneses, then we must assuredly represent these facts in nomenclature and taxonomy. Very likely we shall adopt a scheme wholly different from the current binomial plan for designating one or the other, or perhaps both. We may adopt quantitative names—having determined the main lines of differentiation, may express each variation in names of more or less. I look for some such method to result from the statistical quantitative study of variation. Let me draw an illustration also from plant-breeding practise. The horticulturist and the agriculturist have been holding to the formal or conventional idea of 'variety.' We will suppose that the farmers of a region have grown Jones's Giant White Corn. They have bought and sold and planted this name. They have fed it to the pigs; and the pigs may have thrived or may not, according as the corn contained much or little food value. The name is of no value to the pigs; and, in fact, it is of no real value to the farmer unless it is a guarantee of some particular excellence. Now, the name Jones's Giant White designates corn of certain color and shape of ear and of kernel—features which really mean nothing to the farmer, whereas the starch-content or the protein-content may mean everything. The new plant-breeding does not try to produce a new 'variety' so much as a series of generations that shall have greater efficiency. We shall have, perhaps, fifteen per cent. protein corn, or seventy-

five per cent. starch corn. The name will be of no particular consequence.

If organisms are to be recognized on their merits, then we must cease to class some forms as 'natural' and others as 'artificial.' In the future, the products of the breeder and the plants of the garden are to find their rightful place in systematic plans. They illustrate processes of evolution; and if these processes are hastened by man, the products are all the more worthy of consideration in man-made schemes. The old-time distinction between native forms and domestic forms is arbitrary, unnecessary and pernicious. All animals are animals and all plants are plants.

If we are to designate minor groups or differences in terms of their real qualities, you will still ask how it can be done as a matter of practise, how we shall be able quickly and clearly to determine what particular animal or plant we have in hand. This is really a part of the problem—how to express our ideas without confusion. In the first place, I should say that the change in point of view will come slowly and we shall work out the means as we proceed. I desire only to suggest the direction in which progress seems to lie. In the second place, I should say that in the future we may care less for merely naming a thing than we have in the past—perhaps our formal nomenclature may well stop with characters that are gross and evident. In the third place—and this is the real crux of the matter—I should say that formal nomenclature must never stand in the way of our expressing the full truth about organisms. At best, nomenclature is a makeshift. It is a secondary consideration. If this statement is not accepted, then the only alternative is to say that systems of nomenclature and classification belong to one realm and that biological studies belong to another, and that, therefore, these systems can not be expected to conform to our expanding knowledge. This position would be untenable from the fact that classification is always re-adapting and re-shaping itself to our changing points of view; and nomenclature can not be wholly divorced from taxonomy. Taxonomy represents a progressive effort; nomenclature a conservative effort. Our current phytographic and zoographic methods do not allow us to express our ideas of species.

Every systematist knows how unsatisfactory the mere 'determining' of species is. It consists mainly in matching certain arbitrary characters or marks with similar marks of specimens in the 'collection.' We may have no knowledge whether these marks have any significance in the physiology or phylogeny of the species, that is, whether they are really of any biological value. In theory, we try to hold the systematist to what we call consistency in the determining of species; but as a matter of fact the systematist is constantly changing his mind as to the values of diagnostic marks—and herein, it seems to me, lies the safety of systematic work. A few years ago a botanist sent me a plant to name. When I had returned the name he upbraided me by saying that he had sent me the identical plant the year before and I had then given it another name. I replied that it was his own fault, for he had no business to send me the specimen twice.

The question really comes to this—Shall we know two kinds of species, one of taxonomy and one of biology? If so, then it is scarcely worth while to try to construct any scheme of taxonomy that shall endeavor to express our latest ideas of the ascent of organisms, for a scheme of classification for formal species is needed only for the purpose of ready reference. Comstock has stated the question well in the following paragraphs:*

* 'Evolution and Taxonomy,' Wilder Quarter-Century Book, pp. 44 and 45.

There will also arise, I believe, in a work of this kind, a necessity for distinguishing between the essential characters of a group and those characters which are used by the systematist merely to enable students to recognize members of the group. For it seems to me that the essential characters of a group of organisms do not lie necessarily in the presence or absence of any structure or structures, or in the form or any part or parts of the body of the living members of the group; but rather in the characteristic structure of the progenitor of the group, and in the direction of specialization of the descendants of this progenitor.

The recognition-characters are those usually first observed by the investigator, and are those commonly given in taxonomic works. In many cases these recognition-characters are also essential characters, especially in the case of groups that have been thoroughly studied. But by the taxonomic methods now commonly used, search is chiefly made for recognition-characters. The more skilled the systematist the more likely is he to discover and use as recognition-characters those that are really essential, although the distinction pointed out here may not be recognized by him.

Very likely we shall not abolish the present systems of nomenclature and description in the larger units, but we shall modify and extend them. We shall break away from the old lines of cleavage. We shall learn what marks that are correlated with function can be used as expedient diagnostic characters. We shall make an increasing effort to use absolute characters, not merely relative and comparative ones. We ought to make the 'type' of the species the real biological or phylogenetic type, not cling merely to the 'original' specimen that chanced first to be named. What we now call 'types' may be wholly unusual and even non-significant forms. If the book or literary type is in time to be the real type, then we shall re-group our species-units, and this will be the greatest possible gain.

If we decide that literary-species must come, in the future, to correspond to the physiological or elementary species, then we may hope to express the direction of evolution fairly well in our taxonomic schemes. These taxonomic schemes must proceed centrifugally and dichotomously rather than lineally. They must arrange about foci. I wish to quote again from Comstock:

If the history of a group be worked out in the manner indicated, the student will feel the need of recording his results in such a way as to indicate the phylogeny of the divisions of the group. But as the necessities of book-making require a linear arrangement of descriptions, this is somewhat difficult; for the natural sequence of groups should be represented by constantly branching lines rather than by a single straight line.

It seems to me that the most practicable way of meeting this difficulty is to begin with the description of the most generalized form known, and to follow this with descriptions of forms representing a single line of development, passing successively to more and more specialized forms included in this line. When the treatment of one line of development has been completed, take up another line, beginning with the most generalized member of that line and clearly indicating in the text that a new start has been made.

In making the foregoing suggestions I am well aware that I have not devised any definite nomenclatorial or taxonomic schemes by which they can be carried out. I doubt whether it is worth while to devise any schemes. We need only to establish a few principles and to look upon the present methods as temporary, allowing new methods to grow as our ideas grow. There can be no finality in such schemes or systems. We have lately seen a vigorous revival of the effort towards 'stability' of nomenclature; but nomenclature is only a bit of language, and language can never be stable if it is vital. It was the old idea that systematic work is for the purpose of making record; it is the new idea that it is for the purpose of expressing the meaning of the organic creation.

Ethology and the Mutation Theory: WILLIAM MORTON WHEELER, Curator of Invertebrate Zoology, American Museum of Natural History.

"The mutation theory," as we learn

from the opening sentences of de Vries's celebrated work, "asserts that the characters of organisms are built up out of units distinctly different from one another. These units may be combined to form groups, and in allied species the same units and groups keep recurring. Transitions, however, such as are so abundantly represented in the outer forms of plants and animals no more occur between these units than between the molecules of chemistry." It follows as a corollary from this statement that species must be conceived to arise from preexisting species by discontinuous variations, or mutations, and not by fluctuating variations, or variations proper. The theory is built on a number of remarkable facts derived from breeding organisms, with special attention to their morphological characters or attributes. I have been asked to consider the question as to whether the theory will apply also to the behavior or ethological, as well as to the morphological, aspect of organisms.

The biologist finds it well to distinguish carefully between structure and function, just as the psychologist finds it greatly to his advantage to distinguish sharply between the psychic, on the one hand, and the physiological and morphological, on the other. For the purposes of discussion I will take the standpoint of the biologist in so far as it relates to the distinction between structure and function, but I will combine under function both the physiological and psychological aspects as together constituting ethology, at any rate to the extent that they are involved in the behavior of organisms.

Now, inasmuch as ethology deals with processes, or phenomenal diversity in time, whereas morphology deals with the spatial diversity of phenomena, it is evident that ethological must be very different from morphological characters. It might even be said that the ethologist has no right to speak of a process as a character or characteristic, and the original Greek meaning of these words would seem to limit their use to the structural configurations resulting from specific acts or processes. This need not prevent us, however, from extending the meaning of the terms to include also the typical and specific reactions of the organisms to their environment. Certainly in the case of the human species, which is best known ethologically, the terms character and characteristic are hardly used of physical structures, but almost exclusively of typical modes of activity.

In its application to ethology the mutation theory can only mean that organic species must differ from one another by discrete idiosynerasies of behavior. Most biologists would probably regard any discussion of mutation from the ethological standpoint either as superfluous or as necessarily and merely confirmatory of the results of morphological study. In their opinion it would follow as a matter of course that the functional and ethological characters of organisms must fluctuate or mutate according as the structural characters vary continuously or discontinuously. In my opinion this is not so self-evident as it would appear to be at first sight.

It is true, of course, that the various structural categories from the phylum down to the species, subspecies, variety, sex and individual—all show what may be regarded as correlated or corresponding ethological characters, although this correspondence is often very loose, vague and irregular, for it is evident that slight morphological may be correlated with complex ethological characters, and conversely. Some such correspondence may also be observed in hybrid forms. All this is usually taken for granted, and as a consequence the theory of an ethophysical parallelism, on the model of the famous psychophysical

parallelism seems to have been tacitly accepted by many biologists. If we follow up the matter, however, we soon find that in the field of possible observation the ethological tend to outstrip the morphological characters. We observe great differences in habits and behavior between genera of the same family, between species of the same genus, and what is most significant, between individuals and even twins of the same species. At the same time we may be utterly unable to point out the corresponding structural differences, which, according to any theory of parallelism, should accompany such pronounced ethological distinctions. What bold man, for example, will undertake to show us the morphological characters corresponding to such striking differences in behavior as are manifested by the horse and the ass, by cats or dogs of the same litters, or children of the same parents? Of course, we are at once reminded that there must be corresponding morphological differences represented by cell-structures, biophores, ids, complex chemical compounds, etc. We are compelled to admit that these may exist, but until a function can be shown to be correlated with a particular structure, the structure is, of course, to all intents and purposes a purely hypothetical and imaginary entity. It is clear that the prestige of morphology has been artificially enhanced by a continual appeal to complex invisible structures. Whatever may be the truth concerning such structures, it is undoubtedly a matter of considerable theoretical and practical importance that we are able to detect ethological where we can not detect morphological differences or characters.

We may, in fact, be permitted to reverse the matter and take the point of view of the psychologist and metaphysician rather than that of the morphologist. In other words, we may start with behavior or the dynamic, *i. e.*, physiological and psychological processes of the organism, and regard the structure as their result or objectivation. The organism makes itself—the *ethos* is the organism. In this sense the honeycomb is as much a part of the bee as is her chitinous investment, and the nest is as much a part of the bird as her feathers, and every organism, as a living and acting being, fills a much greater sphere than that which is bounded by its integument.*

Although the time is so very limited, permit me to digress somewhat further on a more practical consequence of the view here advocated. We are certainly justified in regarding ethological characters as very important, as belonging to the organism and as being at least complementary to the morphological characters. If this is true, our existing taxonomy and phylogeny are deplorably defective and one-sided. To classify organisms or to seek to determine their phylogenetic affinities on purely structural grounds can only lead, as it has led in the past, to the trivialities of the species monger and synonym peddler. This has been instinctively felt by all biologists whose development has not been arrested in the puerile specimen-collecting stage.

* Compare, in this connection, the following passages from Schopenhauer's well-known essay on Comparative Anatomy (Ed. Frauenstedt, Bd. 4, pp. 45 and 58): "Man betrachte die zahllosen Gestalten der Thiere. Wie ist doch jedes durchweg nur das Abbild seines Wollens, der sichtbare Ausdruck der Willensbestrebungen, die seinen Charakter ausmachen. * * * Aus meiner Lehre folgt allerdings, dass jedes Wesen sein eigenes Werk ist. Die Natur, die nimmer lügen kann und naiv ist wie das Genie, sagt geradezu das Selbe aus, indem jedes Wesen an einem underen, genau seines Gleichen, nur den Lebensfunken anzündet und dann vor unseren Augen sich selbst macht, den Stoff dazu von Aussen, Form und Bewegung aus sich selbst nehmend; welches man Wachsthum und Entwickelung nennt. So steht auch empirisch jedes Wesen als sein eigenes Werk vor uns. Aber man versteht die Sprache der Natur nicht, weil sie zu einfach ist."

Embryology, by no means a purely morphological science, but one daily assuming a more physiological aspect, has come to have a weighty voice in matters of classification. More recently chorology, or biogeography—a distinctively ethological science—has come to play an equally important part. And rightly, because the organism may be said to seek, and in many cases even to make, its own environment. Every field naturalist knows that he is frequently guided to the more delicate specific and varietal distinctions, not so much by the structural differences between the organisms he is observing, as by differences in their habitat or behavior. Then closer scrutiny may often, although not always, reveal correlated structural differences. When such structural differences are not to be detected we speak of ethological species, and the number of these is undoubtedly much greater than was formerly supposed.* The great reliance on geographical distribution in the more refined taxonomy of certain groups of organisms, like the birds, mammals and social insects, shows an ever-deepening appreciation of ethological characters. It is even jocosely asserted that certain mammalogists are quite unable to identify a specimen unless they are first informed of the exact fence-corner in which it was trapped. Then, and not till then, are they able to perceive the delicate specific or subspecific shade of pelage which goes with life in that particular corner.

The fact that the morphologist has so consistently either neglected or opposed the use of ethological characters in classification shows very clearly that in his heart of hearts he has never very earnestly concerned himself with the parallelism of structure and function. He is inclined to regard function, especially psychical function, as something utterly intangible and capricious. For does it not seem to make its appearance in the embryo or young *after* structure has developed, and to depart at death before the dissolution of visible structure? And are not our museums largely mausoleums of animal and plant structures which we can forever describe and redescribe, tabulate and retabulate, arrange and rearrange, without troubling ourselves in the least about anything so volatile as function?

It is, indeed, not only conceivable, but very desirable, that a taxonomy should be developed in which the ethological will receive ample consideration, if they do not actually take precedence of the morphological characters. It is certainly quite as rational to classify organisms as much by what they do as by the number of their spines and joints, the color of their hairs and feathers, the course of their wing-nervures, etc. To regard our existing purely structural classifications as anything more than the most provisional of makeshifts, is to ignore the fact that the vast majority of organisms which they are designed to cover are known only from a few dead exuviæ. There are, of course, enormous difficulties in the way of constructing ethological classifications, quite apart from the fact that our knowledge of behavior is even more fragmentary than that of structure, as any one will realize who tries to write an ethological description of some common animal or group of animals. In morphology the elements of description can be treated as parts of an orderly and traditionally respected routine, but in ethology we still lack the necessary preliminary analysis of the more complex instincts, and are therefore unable to construct uniform and mutually comparable

* I have in mind a number of cases among insects, such as certain species of ants. There are American forms of the genera *Pheidole, Myrmica, Myrmecocystus, Formica*, etc., which exhibit geographical differences in habits without perceptible morphological differences.

descriptions. One great desideratum in ethology at the present time is a satisfactory and sufficiently elastic working classification of the instincts and reactions, like that of the organs and organ systems of the morphologist. Such a classification can be developed only by comprehensive, comparative study of behavior in a number of genera and families and not by any amount of intensive study of a few reactions in a few species.*

It seemed necessary to discuss ethological characters at some length for the purpose of vindicating their importance. Having attempted this, I may say that these characters seem to me to offer even fewer difficulties than the morphological characters to the acceptance of the mutation theory, for the reason that the ethological and psychological processes are conceived primarily as qualities and not as quantities. Thus the psychical elements, *i. e.*, the simple feelings, cravings and sensations, are disparate qualitative processes which can not be derived from one another or from some more undifferentiated process. This is still more evident in the case of the complex psychical phenomena. Similarly, instincts, with which ethology is most concerned, when resolved into their simplest components are seen to consist of discrete reactions which can not be shown to arise from one another. Although, on the other hand, the measurable intensities and durations of the reactions are analogous to the fluctuating structural variations, it is even more difficult for the psychologist to conceive of a particular feeling, craving or sensation as arising from the greater or less intensity or duration of some other psychic process, than it is for the morphologist to conceive of the origin of new characters from the fluctuating variations of structure.

It is, of course, extremely difficult to determine the first inception of an instinct process, as one may point to the mutational inception of a structural character. An instinct is not an isolated manifestation, but is always more or less influenced by or inextricably bound up with other instincts. Nor do we know of any instinct which manifests itself only in a single species. Still there are numerous cases in which we seem to see more or less clearly the phylogenetic change from one instinct to another. Take, for example, the change from a flesh-eating or insectivorous to a granivorous or vegetarian instinct, a change which has undoubtedly taken place many times in the animal kingdom and is still taking place, especially among insects, birds and mammals. The organs which are useful in obtaining, comminuting and digesting animal food will function with a certain degree of efficiency when vegetable food is substituted, and the animal can pass either at once from animal to vegetable food or through a stage in which both kinds of food are eaten. In the latter case, only after the transition has been completed can we suppose that the organs will begin to assume the more perfect structural adaptations to a vegetarian diet. The state in which the animal is both carnivorous and vegetarian may be regarded as one in which two instincts coexist, and the purely vegetarian is reached by the mutational acquisition of a new and the mutational loss of an old instinct. Undoubtedly many changes of instinct are brought about in this manner so analogous to what has been called in morphology the 'substitution of organs.'

Mutation is even more urgently demanded for the explanation of many other instincts, especially those of symbiotic and parasitic species and of species with pro-

* An avowedly provisional but elaborate 'System der thierischen Triebe' was suggested several years ago by G. H. Schneider in an interesting work ('Der thierische Wille,' Leipzig, Ambr. Abel, 1880), but subsequent workers have not even adopted, to say nothing of having perfected, the schema.

found and sudden metamorphoses. In these cases a particular activity, on which must often depend the life of the individual or of its progeny, has to be performed with a high degree of proficiency at its very phylogenetic inception or it can be of no advantage to the individual or the race. Such cases, with which you are all sufficiently familiar, have ever been the insurmountable obstacle to the evolution of instincts on the theory of fluctuating variations and natural selection. The theory of organic selection seems to me merely to conceal but not to overcome the difficulties. The mutation theory frankly avoids the difficulties even if it fails to throw any light on the origin of the mutations, and bundles this into the germ-plasma. It is, of course, no objection to the theory that it leaves something under the heavens to be accounted for. This is rather to be regarded as one of its chief virtues. As working naturalists we have reason to be most suspicious of the theories that explain everything.

*Discontinuous Variation and the Origin of Species:** Dr. D. T. MacDougal, New York Botanical Garden.

That distinct and separate qualities expressed in recognizable external characters may appear suddenly, or disappear completely, in a series of generations of plants, has been a matter of common observation so long that it would be difficult to hunt out and fix upon the first instance of record.

The significance of such phenomena was obviously beyond the comprehension of the earlier botanists, and it is evident that a rational recognition of the phylogenetic value of sports and anomalies necessarily awaited the development and realization of the conceptions of unit-characters, of the minute structures which are the ultimate bearers of heredity, and of the interdependence of the two in such manner as to constitute actual entities as embodied in Darwin's pangenesis, de Vries' intra-cellular pangenesis and in Mendel's investigations upon heredity. It is equally apparent that a proper interpretation of the facts in question, and their distinction from the results of hybridization were possible only by means of the analysis of the collated results of observations upon series of securely guarded pedigree-cultures, in which the derivation of all of the individuals of several successive generations had been noted. For it is now thoroughly realized that the main questions of descent and heredity and of evolution in general are essentially physiological, and as such their solution is to be sought in experiences with living organisms and not by deductions from illusory 'prima facie' evidence, which has been so much in vogue in evolutionary polemics, nor by 'interpretations of the face of nature' with the accompanying inexact methods and superficial considerations. It was upon the safe basis of the first-named conceptions, and by means of the methods entailed, that de Vries so successfully grappled with the problems involved in the investigation of the part played by discontinuous variation in evolution.

In view of the amount of orderly and well-authenticated evidence now at hand, it may be regarded as demonstrated that characters, and groups of characters, of appreciable physiological value, originate, appear in new combinations or become latent, in hereditary series of organisms, in such manner as to constitute distinct breaks in descent.

This is the main thesis of the mutation theory—the saltatory movements of characters, regardless of the taxonomic value of the resultant forms. That the derivatives

* See also, MacDougal, D. T., 'Discontinuous Variation and the Origin of Species.' *Torreya*. 5: Jan., 1905. Pp. 1–6.

might be considered as species by one systematist, and varieties by another, is quite incidental and of very little importance. The main contention lies in the claim that characters of a definite nature appear, and become inactive suddenly, and do not always need thousands of years for their infinitely slow external realization, or for their gradual disappearance from a strain.

Of course the principal corollary of the mutation-theory is that the saltations in question do result in the production of new species and varieties. As a matter of interest it may be stated that the systematists who have seriously examined the adult mutants of the evening-primroses cultivated in the New York Botanical Garden have unanimously held the opinion that certain ones were to be considered as species and others as varieties.

Furthermore, these conclusions are confirmed when the characters of the mutants are subjected to statistical methods of investigation. In the observations of Dr. Shull, which will be presented more fully before the Botanical Society of America, it has been found that qualities of the mutants, susceptible of measurement, depart definitely and clearly from the parent-type and fluctuate about a new mean, and do not intergrade with the parental form. The amplitude of fluctuation about the new center is greater than that of correspondent parental qualities, and the degree of correlation is much less in the mutants than in the parent. This is seen by inspection to be true in one species during the first year of its existence, and is confirmed by the exact observations on other forms a dozen years after their mutative origin. Consequently the features in question may not be taken to be in any way the result of selection, but are in themselves new qualities.

Lamarck's evening-primrose offers such striking and easily recognizable examples of discontinuous variation, and has been the object of so much detailed study, that we are in danger of giving way to the supposition that the mutation-theory rests upon the facts obtained from this plant alone. It is to be said, however, that if the evidence obtained from it and all of its derivatives were obliterated, the results of experimental studies which have been made upon mutations in other species, upon the behavior of retrograde and ever-sporting varieties, the occurrence of systematic atavism, and of taxonomic anomalies, pelories and other morphological features, would furnish ample support for the conception of unit-characters, and serve to establish the fact that mutations have occurred in a number of species representing diverse groups.

It is now becoming plainly apparent that the phenomena of hybridization, by the opportunities afforded for the study of the included unit-characters in a segregated condition; for the analysis of complex characters, and of the various principles governing the transmission, activity, dominancy, latency and recessivity of characters promises to yield results of the first magnitude concerning the mechanism of descent and heredity. The possibilities, among plants, of crosses between species, comparatively widely different in morphological and physiological constitution, indicate that the ultimate generalizations upon hybridism will find a broader exemplification in plants than in animals.

It is pertinent to point out in this connection that the unguarded use of the terms 'variation' and 'mutation' to designate phenomena of segregation and alternative inheritance, when races or species are thrown together in a hybrid strain, is bound to result in much confusion, especially in dealing with plants, since it is well known that direct mutants of either parent occasionally occur in such mixed strains.

From this last consideration we pass naturally to a discussion of the nature of the material which may be of use in the study of fluctuating and discontinuous variability. It needs no argument to support the assertion that a successful experimental analysis of the behavior of separate characters may be carried out only when dealing with series of organisms fluctuating about a known mean with a measurable amplitude of variability.

Systematic species as ordinarily accepted generally consist of more than one independent and constant subspecies, or elementary species which may not be assumed to interbreed or intergrade, unless actually demonstrated to do so by pedigreed cultures. So far but few elementary species have been found to interbreed. A due recognition of this simple fact would save us a vast amount of pyramidal logic resting on an inverted apex of supposition.

Again, the accumulation of observations upon the prevalence and effect of self- and cross-fertilization has totally unsettled the generalizations current within the last few decades. Briefly stated, a moderate proportion of the flora of any region is autogamous, a large proportion both autogamous and heterogamous, and a moderate proportion entirely heterogamous. The relative number of species included in the categories indicated varies greatly in different regions. To assert the deleterious effects of self-fertilization of all, or a majority of plants, is to base a statement upon evidence that lacks authentication, as has been strikingly demonstrated by recent results. As a matter of fact no phase of evolutionary science is as badly in need of investigation as that which concerns the effects of close- and cross-breeding.

It is also to be said that current misconceptions as to the extreme range of fluctuating variability of many native species have arisen from a failure to recognize the composite nature of the Linnean, or group-species upon which observations have been based, as I have found with the common evening-primrose.

The demands of ordinary floristic work are usually met by the formulation of collective species, which are an undeniable convenience, and perhaps a necessity for the elementary teacher and the amateur. Upon the specialist in any subject rests the obligation to furnish his non-technically trained constituency with conceptions of the facts and principles within the domain of his investigations, which will be inclusive, and easy of comprehension. But if, in accordance with this requirement, the systematist contents himself with this looser, and with due regard it may be said, more superficial treatment, and does not delineate clearly the elementary constituents of a flora, or falters in carrying his analysis of relationships to its logical end, he fails notably in the more serious purpose of his investigations, and his work must be supplemented and extended before it becomes an actual basic contribution to the physiologic, or phylogenetic, branches of the science. To study the behavior of characters we must have them in their simplest combinations. To investigate the origin and activity of species we must have them singly and uncomplicated.

Lastly, we may turn to a phase of the subject which has, as yet, received nothing but speculative consideration—that of the causes which induce the organization of new characters and which stimulate their external appearance. The recurrence of the known mutants of Lamarck's evening-primrose and the occurrence of new mutants of other species have taken place in New York and Amsterdam under conditions that lead to the definite conclusion that a favorable environment, including the most advantageous conditions for

vegetative development and seed-production, facilitates the activation and appearance of latent qualities, and the inference lies near at hand that such conditions also participate as causes in the original organization of new unit-characters, or in changes in these entities. We conclude, therefore, that favorable environment promotes the formation of new species as suggested by Korshinsky, and that new species do not arise under the stress of infra-optimal intensities of external factors as proposed by Darwin.

Furthermore, it has been found that certain qualities arise and disappear more numerously, and presumably more readily, than others, in a mutating strain. Thus those embodied in the mutants *oblonga*, *lata* and *nanella* find external realization in many more individuals than those which constitute the differentiating features of *rubrinervis*, *scintillans*, *gigas*, *elliptica*, *subovata* and others.

Again, the inspection of the cultures made in Amsterdam and New York demonstrates that the last-named locality offers more favorable soil and climate for the evening-primroses. Correlated with this I am able to report that careful attention to the cultures has resulted in an increase of the proportion of mutants from the five per cent. maximum of de Vries to more than six per cent. in the last season, in the American cultures, and to say that some forms which did not reach maturity, and others which did not occur in Amsterdam, may find in New York a climate in which they may carry out their entire development. The cultures of Lamarck's evening-primrose now being carried on include fourteen recognizable mutants, and it is pertinent to state that I have mutants of other species which will be duly described after they have completed a cycle of development.

All components of the environment may not be taken to be of equal value in the induction of new qualities, and I by no means wish to give the impression that the problem is on the point of being solved, but our hopes have been raised to the highest pitch that we may soon be able to discern the factors more or less directly concerned.

To be able to bring the causes that are operative in the formation and structural expression of qualities, *i. e.*, the moving forces of evolution, within the range of experimental investigation would be a triumph worthy the best effort of the naturalist; in that it would give us the power to give new positions to qualities and thus to produce new organisms, its importance would rank well with that of any biological achievement of the last half century.

SCIENTIFIC BOOKS.

The Zoological Record, Vol. XL., being records of zoological literature relating chiefly to the year 1903. Edited (for the Zoological Society of London) by DAVID SHARP. London. 1904. [Published early in 1905.]

The Zoological Record, which has now been published for forty years, is simply invaluable to the working zoologist. This statement seems so trite as to be ridiculous, but the fact is not known to some of those who most need the work. Some time ago I received letters from a well-known naturalist, asking for information concerning a group, the North American species of which he was cataloguing. In my replies I referred to the *Zoological Record;* but the answer came back: 'Do not refer me to the *Zoological Record,* it is not accessible to me.' I do not cite this as an unusual case; on the contrary, one continually observes that writers have not seen the *Record,* and have missed various things in consequence.

Consider for a moment what a time-saver the *Record* is, and how many oversights it prevents. Suppose I want to know about some genus of animals, literally *any* genus that may be mentioned; in half an hour I can find

out what new species have been described in that genus, or important things said about it, in the last ten years. Without the *Record* I might spend days in the quest, and then not find what I wanted.

I have tried the Zurich cards (Concilium Bibliographicum), but have not found them so satisfactory. In a discussion at a meeting of the American Association for the Advancement of Science a few years ago, Professor H. B. Ward stated that for his purposes the cards were preferable to the *Record*, so one must grant that needs differ; yet I can not imagine that a taxonomic worker could do without the *Record*. The cards, as printed, do not list the species separately, although it is now possible to obtain manuscript species-cards for a small sum. Even if cards were printed for all the species, as is done for plants at the Gray Herbarium, I do not think they would be so serviceable as the pages of the *Record*, wherein the eye can pass rapidly over the names of dozens of species, without having to turn over cards. On the other hand, when one has to do with a long series of years, the advantage of an alphabetical arrangement of *all* the species of each genus, and of *all* the genera of each family, is very great; and here, in the long run, the cards will have the advantage. When this time comes, however, it is hoped that the whole will be transferred to printed pages, like the 'Index Kewensis' and Sherborn's 'Index Animalium.' With regard to the International Catalogue, I need say nothing after Professor Ward's recent review (Science, January 27, p. 147); but I was very glad to learn, when in England last summer, that the Zoological Society will not abandon the *Record* while the International Catalogue is conducted on the present basis.

The great superiority of the *Record* is, of course, due to the fact that it is prepared by men who have the most intimate acquaintance with the subjects recorded. In no other way can the work be done satisfactorily, and zoologists ought to feel sincerely grateful that men like D. Sharp, G. A. Boulenger, F. A. Bather, E. A. Smith, R. B. Sharpe and others are willing to labor in their service; to labor, it may be added, for the most trifling pay. Under Dr. Sharp's editorship, the work has greatly improved, and has reached a really marvellous standard of excellence, tested by the groups of which one has particular knowledge. I do not mean to say that there are no errors; but they are remarkably few, and none are due to gross ignorance, like some of those in the International Catalogue. One amusing slip, a few years ago, is worth citing, but it is wholly exceptional. An entomological article was published 'By J. D. Tinsley, A. & M. College, Mesilla Park, N. M.' The Zurich card came in due course, with the article credited to 'J. D. Tinsley and M. College.' I said to myself, that is a blunder the *Zoological Record* would never make; but behold! when it arrived, it contained exactly the same blunder. I dare say M. College will be long remembered as an entomological writer.

Every new volume of the *Record* reminds one of the perennial subject of zoological nomenclature. It does seem that the publication of homonymous generic names is somewhat abating, but I notice in the present volume *Acanthophallus*, Luehe (not Cope, 1893), *Aldrichia*, Theobald (not Coquillett, 1894), *Aporema*, Dall (not Scudder, 1890), *Angelia*, Lower (not Berlese, 1885), *Kirbya*, Melichar (not Desvoidy, 1830), *Nicholsonia*, Tutt (not Kiaer, 1899), *Pleroma*, Melichar (not Sollas, 1888), *Rhynchomys*, Fairmaire (not Thomas, 1895), *Rileya*, Huene (not Howard, 1888), *Thaumasia*, Westerlund (not Perty, 1830), and some others. All these will of course have to be changed. There are also many names which are nearly the same as others, most of which will probably be changed by some one. I am strongly of the opinion, myself, that they should be let alone; as they are distinguishable, and the necessary changes of names are numerous enough. At the same time, it would save trouble if authors would not propose such names. Examples out of the present record are: *Abbottina*, Jordan & Fowler (not *Abbottana*, Hulst), *Asthenoceros*, Laidlaw (not *Asthenoceras*, Buckman), *Biroa*, Bolivar (not *Biroia*, Szep.), *Boccharis*, Distant (not *Bocchoris*, Moore), *Bolla*, Mabille (not *Bollia*, Jones), *Charidia*, Mabille (not *Charidea*, Baly), *Epimela*, Weise (not *Epimelus*,

Edw.), *Greya,* Busck (not *Graya,* Guenth.), *Guerinius,* Ashmead (not *Guerinia,* Bate), *Imeria,* Cameron (not *Imera,* Pascoe), *Ivela,* Swinhoe (not *Ivella,* Lubbock), *Nisia,* Melichar (not *Nisa,* Casey), *Occia,* Tosquinet (not *Occa,* Jord. & Everm.), *Reuterella,* Enderlein (not *Reuteriella,* Signoret).

A few generic and subgeneric names have been omitted; I notice the following: *Crewella* (*An. Mag. N. Hist.,* XII., 202), *Martinella* (*An. Mag. N. Hist.,* XII., 450), *Gueriniella* (Fernald, 'Cat. Coccidæ,' 331), *Kuwania* (*t. c.,* 32), *Kuwanina* (*t. c.,* 121).

It is worth while to say something about the importation of the *Record.* It used to come promptly by mail, but the 1902 volume was just overweight. It was mailed, nevertheless, by the Zoological Society, with the understanding that it would be delivered as before. The British postal authorities took it out of the mails, and turned it over to the American Express Company, with whom they have a contract for the carriage of parcels too heavy for the transatlantic mails. The volume arrived in New York, but was not forwarded until considerable delay had occurred and I had been obliged to pay extra express charges and a heavy import tax. This year, by using lighter paper, I believe, the book was kept just within the specified weight, and it came promptly by mail, with no trouble and no customs dues. It is an outrage to charge duty on a book of this sort, published at a loss, and one would like to know why the charge was made in one case when it was not found necessary in the other.

It is proper to add, that whereas the *Zoological Record* was formerly to be had only as a complete volume, the several subjects may now be purchased separately at moderate prices.

T. D. A. COCKERELL.

Les Lois Naturelles: Réflexions d'un Biologiste sur les Sciences. By F. LE DANTEC. Paris, F. Alcan. 1904. Pp. xvi + 308.

M. Le Dantec has two motives in view: to determine the meaning of the words 'natural law,' and, on the basis of this determination, to define or to revise the main scientific conceptions in use to-day. The meaning of 'natural law' is investigated from a standpoint due to the teachings of biology, with a resulting definition which resembles those of Pearson, Mach, Ostwald, Poincaré and others, and is in substantial accord with the general 'humanistic' philosophy. The author then discusses the meaning of such conceptions as straight line, plane, continuum, mass, force, entropy, absolute zero, inertia, conservation of energy, atom, ether, living matter, thought. With so broad a field to cover, the treatment of each conception must needs be brief; but it is at least direct, systematic and clear.

In the introduction (16 pp.) the general considerations are laid down which will determine the author's definition of natural law. Of the external world we know only the ways in which it affects us, the relations it bears to us. These ways or relations come to us through several gateways—namely, the senses —which the author calls the 'sensorial cantons' (sight, touch, temperature, smell, taste, etc.). Of these there are, we are told, many more than physiology admits, though we are not given a complete list of them. They are each irreducible, inexpressible in terms of any other sense. What we see has form and color, but is not loud nor hot; temperature has no color nor sweetness, tastes are not square nor round. What is revealed to one sense can not properly be described in terms of any other sense. Now science is first of all a record of these quite different classes of sense-impressions.

The subject is continued in Book I., 'The Sensorial Cantons and Monism.' Man is not only passive toward the external world. He reacts upon his environment; and in order to do so he makes hypotheses about the constitution of that environment. In the early stages of man's development these are quite as likely to be useless as not, but natural selection preserves the useful and weeds out the useless, till in the course of ages the former become instinctive. Thus our instinctive belief that arithmetic is infallibly correct, or that unsupported bodies fall, is the 'hereditary résumé of ancestral experience' (p. 3). We regard it as an *a priori* truth because the belief has been so long perpetuated by natural selection.

This is the meaning of natural law, and the extent of its validity and the validity of all our reasoning must be judged in the light of this meaning. Our elementary laws of motion can not be assumed to hold of bodies too small for observation, or too remote in space. Science is not absolute in any sense (p. 8). But not only do we react on our environment in accord with many acquired beliefs; we seek economy in our reactions and our beliefs. We find it easier to indicate what will happen in the various sensorial cantons by using the terms of one sense, namely, sight. Because sight gives greater precision and covers a wider field of phenomena than any other sense, we find it most convenient to frame our hypotheses in regard to the constitution of the other cantons in terms of what sight reveals—namely, the motion of bodies. Thus we describe sound, heat, light as wave-motion. It is the hope of covering all the phenomena of the other cantons by the terminology of sight that leads to monism. Yet what is vouchsafed by the various senses remains really disparate, *sui generis* in each canton.

In Book II. we consider the sciences of the 'optical canton'—more familiarly known as the exact sciences. The language of mathematics is the language of vision. It is based directly on sense-impressions. There is no 'free creation by the mind' of the fundamental mathematical conceptions. The straight line is given in the thread suspending a weight, the plane in the surface of a liquid, the continuum in any body in which we see no gaps. These sense-impressions may turn out later to be illusions, but are none the less really given. Arithmetic, algebra and geometry detail the properties of such data omitting the element of time; kinematics and mechanics include the latter. The infinite and infinitesimal are not picturable, therefore they are figures of speech. Nor could we tell what laws they would obey, since logic is based on what we have experienced, and can not be assumed to hold of regions beyond our senses. Just so, atoms may not obey the laws which larger bodies obey, and the ether may not be impenetrable.

How do we come to use other conceptions here besides those of space and motion? Because these are found insufficient for the prediction of the behavior of bodies. Thus we find it convenient to speak of mass, which the author defines as 'that coefficient found in one and the same body, in all the systems of which it forms a part' (p. 87). Velocity is that which corresponds to the intensity of our sensation of motion (p. 94). Force is not a cause of motion; for such a conception imports the muscular sense into the visual field, which is not allowed. Quantity does not apply beyond the visual field. Incidentally, Fechner's law is declared impossible, since it attributes quantity to other cantons besides the visual. What we really measure is not force as an existing quantity of something, but only $m \times a$, a numerical product. That force is a fiction is shown also when we remember that in statics any one force may be replaced by an infinity of others equivalent to it. Since all these can not be really present, there is no reason for saying any one is more real than the rest. The law of action and reaction is the experimental fact that in an isolated system the algebraic sum of the partial energies is *nil*. The conservation of mass has become so well known as to be almost an *a priori* law.

In Book III., 'The Other Cantons,' the discussion is confined to sound and heat, principally to heat. Temperature can be studied scientifically because it alters the shape of bodies. The conservation of heat is simply a definition of a complete system: a complete system is one in which the algebraic sum of the quantities of heat gained by the parts is *nil* (p. 148). 'Source of heat' and 'absolute zero,' like force, are fictions. Equivalence of heat and mechanical energy does not mean preservation of a permanent something; it is only a useful device for correlating heat with visual phenomena. The conservation of energy is only such a correlation made general; it is an empirical truth at most, and even if radium creates energy *de novo* it need not disturb us (pp. 207–9).

Book IV., 'Explanations,' resumes the general position of the author in regard to the meaning of scientific law. Atomic models do

not explain; they simply enable us to take points of view fertile for discovery of new properties. Thus atomism is to be preferred to energetics, the latter, though nearer to fact and less liable to dangerous hypotheses, does not stimulate the mind to discovery. Better danger than the precision of sterility! (p. 229).

Biology (Book V., 'The Place of Biology among the Sciences') the author would regard as underlying zoology, botany, physiology, etc., even as theoretical mechanics underlies physics. As theoretical mechanics defines the motion of bodies, biology defines life, leaving to the detailed sciences the description of different forms of life. Life itself is defined as a localized process, like the flame, not a specific substance or energy, but a locus of points where certain reactions are accomplished. The characteristic property of life is assimilation (p. 288). Thought and other psychoses are described as a special sensorial canton 'le canton intime.'

The general position of the author, that sense-impressions are all we know, and that the sources of heat, light, sound, etc., are not *in themselves* describable in visual terms, is an extreme one and is open to all the objections which are being urged, rightly or wrongly, against 'humanism.' But further, it is quite dogmatic to say that quantity does not apply beyond the visual field, or that mathematics is the language of vision; what is needed is a more exhaustive account of the conceptions involved. It is also to be regretted that M. Le Dantec, as a biologist, has not made use of the discoveries of Mendel, De Vries and others, which inevitably suggest that the fundamental law of science is not mere determinism, as he says (p. 213), but chance (in the mathematical sense) as well. Nevertheless, the attempt of M. Le Dantec to give clear and concise definition of the principal scientific conceptions should be welcomed by scientists and philosophers alike, and should lead to further work in the same direction.

W. H. SHELDON.

COLUMBIA UNIVERSITY.

SOCIETIES AND ACADEMIES.

THE NEW YORK SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE section held its fifth regular meeting of the season at the Chemists' Club, Friday evening, February 10.

The president of the American Chemical Society, Dr. Francis P. Venable, presented to Professor Charles Lathrop Parsons, of the New Hampshire College, Durham, N. H., the Nichols medal, which was awarded to him for his paper entitled 'A Revision of the Atomic Weight of Beryllium,' read before the section in May, 1904. Mr. W. H. Nichols, the donor of the medal, was also present and made a few appropriate remarks.

The regular program of the evening was then taken up and the following papers presented:

The Accumulation and Utilization of Atmospheric Nitrogen in the Soil: E. B. VOORHEES and J. G. LIPMAN.

The experiments planned included, first, a study of the question of the sources of nitrogen to leguminous plants on soils to which no nitrogen had been applied, and to which nitrogen in various forms and amounts had been applied; second, the availability of cow pea nitrogen, as compared with the different forms of nitrogen for the growth of non-legumes; and third, the possibility of the accumulation of nitrogen in cultivated but uncropped soils.

The soils used were light in character, poor in nitrogen and supplied with an abundance of the mineral elements.

Briefly, the results show that the cow pea crop accumulated large quantities of nitrogen, and that the greatest accumulation was where no nitrogenous materials had been applied. Or, in other words, that the addition of the nitrogen decreased rather than increased the content of soil nitrogen, indicating that the leguminous crop will accumulate proportionately larger quantities of nitrogen upon soils relatively free from this element.

Millet was then grown two seasons, both upon the soils upon which the cow pea had grown, and upon which no crops had been

grown, and without further application of nitrogen.

The final analyses of the crops and soils showed that the organic nitrogen contained in the cow peas, and accumulated largely from the air, was utilized by the succeeding non-leguminous crop, and that there was a gain of nitrogen in the soils where no cow peas were grown, and that such gains were greatest where the largest amount of manure had been applied.

The Detection of Methyl Alcohol: Heyward Scudder.

The formation of methyl salicylate is not a reliable test, because ethyl salicylate has almost the same odor. The Riche and Bardy test takes too long to be of practical value. The Trillat test (delicacy 0.2 per cent.) is uncertain. The deepening of the blue on heating is characteristic. Wolf and Robine state that Trillat's test is not reliable and give modifications that may be of value in cases of doubt. The Haigh (delicacy 5 per cent.) and Spanglé-Ferrière (delicacy 1 per cent.) tests use phloroglucine to show the presence of formaldehyde. The color obtained varies with the concentration of the formaldehyde. A blank test with ethyl alcohol must always be made. The effect of heat is characteristic. By using proper conditions a rapid test (delicacy 2 per cent.) can be made. The Mulliken, Scudder test (delicacy 3 per cent.) is very reliable because of the formation of flocks as well as color. On account of the uncertainty of color tests for small amounts, it is advised first to try a rapid test and, if no definite result is obtained to concentrate by fractionation.

The Origin of Radium: Bertram B. Boltwood.

It had been shown in previous papers that the determination of the relative quantities of uranium and radium in a number of minerals strongly indicates a constant ratio between the quantities of these elements occurring together. In the present paper improved methods for the quantitative determination of both radium and uranium are described, and the results obtained from an examination of twenty-two separate samples, comprising twelve distinct mineral species, are given. These results show a constant proportionality between the quantities of radium and uranium in all of the samples examined, and lead to the inevitable conclusion that uranium is the parent of radium.

F. H. Pough,
Secretary.

Discussion and Correspondence.

The Naturalist's Universal Directory.

A new edition of the 'Naturalist's Directory' has recently come to hand. In casually turning the leaves I noticed that the names of Joseph Le Conte and J. W. Powell were still retained in the list of American naturalists, although their deaths had occurred as long ago as 1901 and 1902. These lapses suggested that the directory might not be trustworthy in other respects, and at my suggestion an associate compared its lists with various other lists of scientific men, for the purpose of testing its accuracy and fullness. The practical utility of Cassino's directory in the past seems to be attested by the fact that it has reached its nineteenth edition, and I therefore feel justified in presenting, as a matter of general information, some of the results of the examination.

In order to judge of its fullness a comparison was made with the contemporary lists of a few scientific organizations whose membership is carefully selected on the basis of scientific ability or accomplishment. Of 90 members of the National Academy of Sciences the directory fails to include 28. It omits 129 of the 329 members of the Washington Academy of Sciences, 57 of the 233 members of the American Society of Naturalists, 11 of the 46 fellows of the American Ornithologists Union, and 54 of the 259 fellows of the Geological Society of America. Probably some members of each organization are not within the scope of the directory, but this remark can not apply to such men as Outram Bangs, C. F. Batchelder, Lyman Belding, Franz Boas, Lewis Boss, A. P. Chadbourne, C. F. Chandler, S. C. Chandler, W. W. Cooke, G. A. Dorsey, Wm. Dutcher, H. G. Dyar, H. W. Fairbanks, W. G. Farlow, J. W. Fewkes, Henry Gannett, A. C. Gill, L.

P. Gratacap, C. S. Hastings, J. P. Iddings, C. L. Jackson, S. P. Johnson, S. C. Keith, N. T. Lawrence, G. Lefevre, C. K. Leith, E. W. McBride, L. B. Mendel, T. C. Mendenhall, A. A. Michelson, John Muir, E. W. Nelson, E. L. Nichols, A. E. Ortmann, Wm. Palmer, H. S. Pritchett, T. M. Prudden, H. A. Purdie, E. F. Smith, J. C. Smock, R. Thaxter, O. H. Tittmann, John Trowbridge, W. L. Underwood, Lester F. Ward, A. G. Webster, E. L. Wells, C. A. White, S. W. Williston, H. C. Wood and R. R. Wright. It was not practicable to apply a similar test to the foreign lists, and it may be that they are fuller.

To test the accuracy of the addresses given they were compared with lists, of approximately the same date, published by the Washington Academy of Sciences, the Geological Society of America, the American Society of Naturalists and the American Ornithologists Union, and with 400 other addresses taken at random from 'Who's Who in America' and the lists of the American Association and the affiliated societies of Washington. In all about 750 addresses were compared, and it was found that about ten per cent. of those given by the directory are erroneous. Similar comparison was made of 291 names common to the directory and the list of the Geological Society of London, with the result that 52 addresses were found to be discrepant, but in this case it was not possible to say how many were wrong.

Of deceased scientists so notable that their deaths are recorded in the necrologies of the National Academy, the *American Journal of Science*, or the American 'Who's Who,' no less than 49 are retained by the directory. Among these are not only Powell and Le Conte, already noted, but Elliott Coues, Horatio Hale, James Hall, J. Willard Gibbs, St. George Mivart, Henry Morton, A. E. Nordenskiold, H. A. Rowland and Rudolph Virchow.

The arrangement of the names is by countries, with a classification which has been gradually evolved through successive editions. Part I. comprises, first, the United States and Canada, and then, in order, Great Britain, Central America, South America, Oceanica and Africa. Central America is made to include not only the usual states, but Mexico, Newfoundland and the islands of the West Indies; and the countries of Asia are placed under Oceanica. Part II. includes all the countries of Europe except Great Britain. The use of Part II. is facilitated by having its parts arranged in alphabetic order, and by the insertion of the name of the country at the head of each page; but these devices are not used in Part I. In some of the earlier editions the entries for the United States and Canada were numbered *seriatim* and a special index of departments of science referred to these numbers. From the present edition the index is omitted, but the numbers survive as a vestigial character.

The personal list for the United States and Canada is followed by a list of scientific societies of the same countries, with a classification by states. Being a resident of Washington, I turned, naturally, to the list for the District of Columbia, and noted at once the omission of the Washington Academy of Sciences and of nine out of the twelve scientific societies affiliated with it. Of the three affiliated societies that are listed the data for two are obsolete. The American Association for the Advancement of Science, which for seven years has had its headquarters in Washington, is still credited to Salem, the place of publication of the directory.

Despite these limitations the directory is a useful volume. It contains the names of about 18,500 scientists, with information as to addresses and specialties, and the greater part of the information is correct.

G. K. GILBERT.

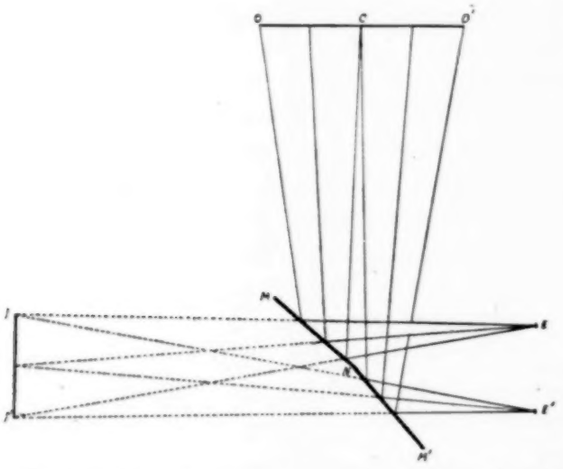
AN OVERLOOKED FORM OF STEREOSCOPE.

IN SCIENCE of November 18, 1904, Professor Jastrow describes, under the above heading, an ingenious modification of the mirror stereoscope, permitting the use of the ordinary stereoscopic card.

The arrangement described below, serving the same purpose, appears to possess some advantages. It is quite possible that this form may have been suggested before, but it has not come under my notice.

In all arrangements of this kind it is of

advantage to bring the mirrors as near to the eyes as possible, as was done in Sir Charles Wheatstone's original arrangement. If we think of the mirrors, as we may from an optical standpoint, as simple openings through



OCO' is the stereoscopic card. *MN* and *NM'* are the two mirrors. *II'* are the superimposed images of *OC* and *CO'*, and *E*, *E'*, are the positions of the two eyes.

which we see the image, it is evident that the eye gains much in freedom of position and largeness of field, if it is brought near to the mirror, precisely as we go close to a window if we desire a more extended view. Also, since the edge dividing the mirrors is thus brought between the eyes, rather than in front of them, it is no longer seen, or at least is no longer troublesome, as it is to some extent when the mirrors are farther from the eyes. Indeed, the mirrors need not meet at all nor need they be of any regular shape.

With this arrangement two views may be combined which are considerably wider than those used in the ordinary stereoscope. I have found no difficulty with drawings six inches wide. The height of an object which can be successfully used is limited by the condition discussed below. But with views of the ordinary dimensions this stereoscope is entirely satisfactory in its performance, possesses a considerable range of adjustment, and is convenient for laboratory experiment, as it is easily and quickly put together with two bits of mirror and a little wax.

All forms of reflecting stereoscope using a single stereoscopic card have this imperfection in common, that the images formed by the two mirrors do not coincide, but intersect at a considerable angle. The images of any object formed by two mirrors lie, as is well known, on the circumference of a circle, the center of which is at the junction of the mirrors, and the images are separated by an angle equal to twice the angle between the mirrors. Since the relations between object and image are reciprocal, it is plain that if the images of two objects are to be superposed by means of mirrors, forming one image, the objects must lie on the circumference of the circle, and at the angular distance occupied by the images in the previous case.

If this condition is not fulfilled, but the objects are in the same plane, as when they are on the same card, the images, while nearly superposed, will intersect at an angle equal to twice the angle between the mirrors. In my stereoscope, as commonly used, the angle between the mirrors is about two and one half degrees, so that the images form an angle with each other of five degrees. The images are over three inches wide. If their planes intersect at the median line, the edges to the right and left are separated in the direction of the line of sight by more than an eighth of an inch. This is hardly noticeable across the breadth of the view, where the line of sight is nearly perpendicular to the intersecting edge of the mirrors, but becomes so near the top and bottom, where the slight deficiency in sharpness of the horizontal lines is easily traceable to their inclination. If the center of the card is pushed back so that the card forms an arc, approximately that of the circle on which the views should lie, the improvement in definition is strikingly evident.

It is an interesting illustration of the ease with which the eye is satisfied in such matters, that the stereoscopic result is excellent over the whole view, hardly failing at all even at the extreme edges, though formed by two images so disadvantageously placed.

Frank P. Whitman.
Western Reserve University,
Cleveland, Ohio.

KILAUEA AGAIN ACTIVE.

IT may be of interest to the readers of SCIENCE to know that the fire has again returned to the world-renowned volcano Kilauea in the Hawaiian Islands after an absence of thirteen years. The citizens of Hawaii, who are intensely interested in this volcano, had well nigh despaired of witnessing another season of activity. The fresh lava appeared the last week of February, heralded by a slight earthquake. On the twenty-fifth instant it was not observed—smoke filling the pit. Two days later it is reported that a lava lake was on exhibition, two hundred and fifty feet long and one hundred feet wide. On March 10 the Volcano House reported that the lake is not so large as at first stated; but the crater is absolutely free from smoke. "Heavy rumblings and explosions indicate that another outbreak is imminent." Thus there seems to be a restoration of the old-time activity—such as will cause a large increase in the number of visitors.

Observations with a good spectroscope are needed. Professor Libbey used one there to good purpose a few years since, but did not name all the substances indicated. We especially need more information about the hydrogen flames, as well as the hydrocarbons. The latter substance is so commonly of organic origin that the best of evidence is required to fully establish a belief in its presence in this incandescent magma fresh from the realms of Pluto. It is hoped that some one who is skilled in the use of the spectroscope will utilize this opportunity to determine the nature of the substances now being emitted from this famous volcano. C. H. HITCHCOCK.

HANOVER, N. H.,
March 23, 1905.

SPECIAL ARTICLES.

THE PRAIRIE MOUNDS OF LOUISIANA.

WHILE it may not generally be appropriate to discuss the content of a paper on the basis of a mere abstract report by the secretary of a society, I venture to make some comments on the paper read by A. C. Veatch on the 'Natural Mounds of Louisiana,' at the late meeting of the Geological Society of Washington, as given in the last issue of SCIENCE; since I have made a number of such excavations as are called for by him.

I have briefly discussed these mounds in my final report on the geological reconnoissance of Louisiana made by me 1869, published in 1873. I dug into a number of them on the Opelousas prairie, and also on the Calcasieu prairie. Having just previously investigated the mud-lumps of the Mississippi Passes, my first conjecture was that of mudspring origin; but the total absence of the characteristic 'onion' structure of such mudspring cones at once made me abandon this hypothesis. The total absence of any regular structure or stratification, such as characterizes all dune or other wind-drift structures, equally excluded these; as well as water erosion, since the soil and subsoil of the surrounding prairie are quite distinctly in horizontal layers. I, therefore, as shown in the paper alluded to, considered their ant-hill origin as the only reasonable explanation; raising the question as to how the once teeming population of these vast areas came to be destroyed. Climatic changes suggested themselves to me, but the present existence of ant villages in the adjoining state of Texas seemed to negative this assumption also.

A number of years afterwards I was forcibly reminded of the inutility of supposing climatic changes to have occurred, when having camped in the Yellowstone valley after nightfall on a convenient elevation above the sodden ground, I was put to precipitate flight by an army of large ants issuing from beneath my rubber mattress. Daylight observation revealed to me the counterparts of the Louisiana mounds, only as a rule less thickly grouped than on the Louisiana prairies; and on excavating some of these mounds which had been deserted by their aggressive inhabitants, I noted precisely the same structureless earth I had seen in the Opelousas prairie, only this time traversed by half-obliterated burrows, which in the Louisiana mound-fields were almost wholly imperceptible, or at least undistinguishable from old root-tracks.

It therefore seems to me that the question of the Louisiana mounds resolves itself into a biological problem, viz., what kind of ant

might have built up these elevations, and what causes might have operated to depopulate them. For if mound-building ants now live both in Texas and in Montana, it is hardly necessary to call in climatic changes to account for the facts.

In California there are extensive tracts of similarly appearing mounds (*vulgo* 'hogwallows') in the San Joaquin valley; but here not only can their wind-drift origin be substantiated by ocular demonstration during any of the frequent sandstorms, when the sagebrush clumps are often left two feet above the general level because their roots resist the eroding action by holding the sand; but the wind-drift origin of the general soil surface can mostly be verified, even when, as frequently happens, the bushes thus left 'high and dry' die out in the course of time, and subsequent aqueous erosion increases the height, and a gradual consolidation of the material occurs.

'Hogwallows' of quite different origin occur in Washington, on the gravelly lands south of Tacoma city, *e. g.*, on Yelms prairie. Here, in the land of almost daily heavy rains during certain seasons, water erosion has removed the sand and smaller gravel from variously-shaped areas surrounding one or several larger blocks (erratics), the channels between adjacent mounds being lined with cobbles left behind by the water. Yet while the general aspect of the surface is similar to that of the 'hogwallows' of California and the mounds of the Calcasieu prairie, there is clearly no genetic relation between the three kinds of 'mounds,' however similar in their external conformation.

E. W. Hilgard.

Berkeley, Cal.,
March 10, 1905.

Progress in the Study of the Kelep.

The existence of an efficient insect enemy of the boll weevil having been ascertained, it became necessary to determine also the extent, if any, to which it could be utilized in the United States. Since the last published report on the subject* many additional data have accumulated, but there are three features worthy of special notice.

The Kelep in Western Guatemala.—Recent letters from Mr. W. R. Maxon report the existence of the kelep in the cotton fields of the Retalhuleu district of western Guatemala. Mr. Maxon has also sent to the Department of Agriculture seeds and dried bolls of an upland cotton of a variety evidently similar to that grown by the Kekchi Indians of Alta Vera Paz. The three nectaries inside the bracts appear to be even larger than in the Kekchi cotton. The pair of inner stipular bracts which subtend each of these nectaries are the largest yet known, and have their margins fringed with long hairs, as though to increase their efficiency in holding the nectar to attract the keleps inside the involucre.

This west Guatemalan or so-called *Pachon* cotton is also an annual crop and is said to ripen in five months, or in even less time than the Kekchi. Following the analogy of other plants, these varieties, if they can be acclimatized in the United States, may be expected to mature in a still shorter period, which gives them distinct agricultural interest. The effectiveness of the plan of mitigating the injuries of the boll weevil by cultural means depends upon the shortening, as far as possible, of the growing season of the cotton plant. Other things being equal, a short-season variety will also be an early variety, of course, but the simultaneous planting of quick-growing varieties is likely to prove a better measure of protection than uncertain and desultory early planting, because the weevils are much more likely to perish by starvation after the weather is warm enough to bring them to the condition of activity than while they are kept by the cold in a torpid, hibernating state. It is thus not impossible that these short-season varieties of cotton which are cultivated in Guatemala by the help of the kelep may prove to be of value in the United States, even without their insect guardians. It may be repeated, too, that both of the dwarf, kelep-protected varieties of Guatemala belong to the upland type and produce fiber of good length and quality.

* 'Report on the Habits of the Kelep,' Bull. 49, Bureau of Entomology, U. S. Department of Agriculture, 1904.

Breeding Habits of the Kelep.—The continued study of the kelep in Guatemala, as well as in the United States, makes it possible to outline the breeding habits of the insect, which are in many respects different from those of the true ants, and very much more suited to the purposes of domestication. Popular language affords only the one word 'ants' for all the wingless, social insects. Even the termites are everywhere called 'wood ants' or 'white ants,' though having no affinity or similarity with the true ants apart from their social habits and the winglessness of the worker castes.

The kelep furnishes another chapter of the same kind of history. It has, apparently, little or nothing to do with the true ants. Its social economy does not follow the monarchical system of the ants and termites, but represents an entirely different system, more like that of the honey-bees, in that new colonies are founded by the subdivision of the workers of older communities instead of by solitary queens. The keleps, indeed, have carried the principle of organization a step further than the bees, for they do not depend upon the queen to lead the swarm, but take her by the jaw and carry her over to the new burrow, in case she fails to go voluntarily. The new establishment is also equipped with eggs and larvæ brought over from the old, so that the founding of a new colony does not involve any interruption of the domestic activities. This mobile organization of the keleps suggests to a slight extent the social habits of the nomadic driver 'ants,' and as with these the queens are stationary, and probably never leave the nest except when carried by the workers in migrating to the new home. Males are to be found in the kelep nests throughout the year and cross-fertilization is probably accomplished by the wandering of these from one to another of the closely adjacent, not unfriendly communities. The kelep queens have wings at first, but probably never use them. In some of the related genera the queens are quite wingless, as among the drivers.

Ability to Withstand Cold.—A cold storage experiment made in Washington last August showed that the keleps would be able to withstand low temperatures, and the colonies which have been left in the Texas cotton fields through the winter have lived long enough to show that cold weather is not likely to be the insurmountable obstacle which will prevent the establishment of this species in the United States.

Recent advices indicate, however, that none of the field colonies which have received no food or care of any kind since they were placed in the ground last July will survive to the cotton planting season. A sufficient cause for this mortality is doubtless to be found in starvation, though other contributing factors are apparent, now that the social organization of the insects is better understood. The season has been, as is well known, one of unusual severity, both in drought and cold. The colonies were planted in the fields so late in the season that it seemed necessary to look for the dampest places in order to give them a fair chance to dig, but this has exposed them to special danger from flooding, which appears to have been the immediate cause of death in several instances, and possibly in all. The fields in the vicinity of Victoria, where most of the colonies were located, were completely and repeatedly denuded of their foliage, flowers, buds and young bolls by the leaf worms. Although the keleps readily captured and made use of these when the successive broods were in the larval condition, and even broke into the pupæ, there were intervals when food was almost entirely lacking, and even boll weevils became extremely scarce.

There are also two important social causes of demoralization. The colonies were brought from Guatemala under the impression that the keleps were true ants, and would be able to replenish themselves if the queens were secured. Our artificial nests were mostly very small, no jars of suitable size being obtainable in that part of Guatemala. The complement of workers was, therefore, usually very much below the normal. It has since been learned by repeated observation that the keleps are like the honey-bees, in that the reduction of the colonies below the normal size induces discouragement, dejection and aberration of instincts. Neither did we take pains to include

with all the colonies eggs, larvæ and pupæ. The queens lay freely in captivity, but a break in the normal succession of forms may be disastrous, because nurse duty is performed by the young, light-colored workers, the predaceous, hunting instincts appearing with greater maturity. The neglect of the young in some colonies and a frequent tendency to cannibalism may be ascribed to this deficiency of keleps of proper age, though even in colonies otherwise normal some of the larvæ are occasionally killed and fed to the others, especially if there has been a deficiency of other animal food.

Detailed reports on the social organization and other features will be made, but in the meantime it is apparent that a fair experiment to determine whether the kelep can maintain itself in the United States will require the planting of full-sized colonies early in the season, and in sufficient numbers, if possible, to protect the field of cotton from the leaf-worms as well as from the boll weevils. That the kelep is not a true ant, and that its habits differ so greatly from those of any other insects previously known, are facts that show how impracticable it would have been to determine its possibilities in advance by the application of analogies drawn from insects of other families.

O. F. Cook.

New Orleans,
March 16, 1905.

QUOTATIONS.

The Sanitation of the Panama Canal Zone.

Dr. Charles A. L. Reed, chairman of the legislative committee of the American Medical Association, and lately president of this the representative organization of the medical profession throughout the United States, a man capable in every way of forming just conclusions and with the courage and capacity vigorously to express his convictions, has, at the request of the Secretary of War, submitted a report of the sanitary, or unsanitary, conditions in the Panama Canal Zone and in the towns of Panama and Colon placed by treaty under the management of the United States commission.

If the report of this gentleman is correct, and it must be accepted as such until it is proved that he has made misstatements, the sanitary department of the Panama commission has been in great measure paralyzed by circumlocution and red tape and the misguided interference of those who have been placed in authority over the medical corps. In certain quarters Dr. Reed's report has been classified as 'frenzied' literature; but if one-tenth of the criticisms which he has made were justified sanitary affairs on the Isthmus are in such deplorable shape that the president should compel an immediate change in a disgraceful and dangerous situation.

There is but one commonsense solution of this problem, and sooner or later it will be applied; but the chief magistrate should not wait to take this matter in hand until the graveyards of Panama are filled with the victims of 'red tape.'

As an illustration of the absurd methods employed, Dr. Reed says in substance that if the surgeon in charge of the Ancon Hospital makes a requisition for supplies it must go to the chief sanitary officer for approval, then to the governor of the zone, then to the chief disbursing officer and thence to the commission at Washington. It must there wait for advertised bids, and when the award is made the requisition is filled under the supervision of a purchasing agent, often not properly qualified to select medical supplies. The material is then shipped to the Isthmus, the disbursing officer is notified, he notifies Col. Gorgas, and he in turn must notify the surgeon in charge of the hospital, who then applies to the quartermaster for transportation; and, finally, so much of the material as in the judgment of the governor and chief disbursing officer and the commission ought to be allowed to the superintendent arrives at the hospital.

There are cited numerous other instances of this ridiculous routine which in the light of a recent experience are a reflection upon the intelligence and conscience of the American people.

We refer to Cuba, where Major Reed and Col. Gorgas practically had *carte blanche* to do what in their judgment was best for the sanitation of Havana and Cuba.

The present situation can be remedied satisfactorily in but one way. Col. W. C. Gorgas is known to be one of the most expert sanitarians now living. He is a man whose courage is of that exalted character which scorns personal danger, a man of integrity, of executive ability and worthy of the fullest confidence of the government. Why not repeat the experience of Cuba in Panama? Why go back to the old methods of crippling the usefulness of the Army Medical Corps by permitting it to be blocked by circumlocution or entangled in the meshes of red tape until it might almost as well not exist?

The people of the United States will oppose the president's removal of the present commission, and if he will go further and put an end to this dangerous condition of affairs by placing Col. Gorgas in full authority in all matters pertaining to sanitation he will deserve still greater credit. In the construction of the Panama Canal the question of sanitation is paramount.—The N. Y. *Sun*.

BOTANICAL NOTES.

A HELPFUL BULLETIN.

THE office of Experiment Stations of the United States Department of Agriculture has issued a Bulletin (No. 2) consisting of an outline of a lecture on 'Potato Diseases and Their Treatment' for the use of farmers' institute lecturers. It was prepared by F. C. Stewart and H. J. Eustace, of the New York Experiment Station. It contains summaries of our knowledge of the most important diseases which affect the potato in the United States. The descriptions are given in non-technical language, and ought to convince every botanist of the possibility of treating quite difficult subjects in plain English. Following the description of diseases is an admirable chapter on spraying and other preventive measures. A very useful bibliography is added in an appendix.

SEASIDE LABORATORIES.

IT is a fortunate thing for the scientific students of America that year by year the opportunities for seaside study are more common and easily accessible. Some of us remember the time, not so very long ago either, when Agassiz's laboratory on Penikese Island was the only place where seaside studies were possible under competent guidance and supervision. The Penikese laboratory has long since ceased to be—on the death of its illustrious founder it could not secure adequate support. It died, and men spoke of it as another visionary project which had met with the usual fate of an early death after a brief and fitful existence. But although that project died, others have arisen to more than take its place. To-day laboratories that include the essential features of the one founded by Agassiz are not uncommon on both coasts of the United States, as well as on the shores of our inland waters.

The eighteenth session of the Marine Biological Laboratory at Woods Hole, Mass., is of interest to the botanist not only on account of the botanical courses offered, but also because this is to a certain extent the lineal descendant of the Penikese laboratory whose abandoned site is but a few miles away. As heretofore, the work in botany is to be under the direction of Professor Doctor Bradley M. Davis, of the University of Chicago.

There are the usual opportunities for investigation for advanced students, and regular instruction in the morphology of thallophytes, cytological studies and plant physiology. The laboratory is open for investigation from June 1 to October 1, and for instruction from June 28 to August 9.

On the west coast of Vancouver Island, at Port Renfrew, twenty-six hundred miles from the Woods Hole laboratory, is the Minnesota Seaside Station, whose fifth session is announced for the present year, under the directorship of Professor Conway MacMillan, of the University of Minnesota. Although so far away from the pioneer Penikese laboratory, this one on Vancouver Island is filled also with the spirit of the master who taught us to study nature out of doors. Here, in addition to opportunities for investigation, botanical courses are offered in algology, lichenology, bacteriology, taxonomy of the Coniferæ and nature study. The session begins July 8 and closes August 18.

It is interesting to compare these two marine stations—the first on the New England mainland, looking across to Martha's Vineyard, and the Elizabeth Islands, with the interesting flora of the north Atlantic coast, rich in many red seaweeds; and the second on the shore of Vancouver Island, looking out across the Strait of Juan de Fuca, to Cape Flattery, and the Olympic Mountains of the northwest corner of Washington. Here the marine flora is especially rich in the great kelps so characteristic of the Pacific coast of North America.

UTAH FUNGI.

Some months ago Mr. A. O. Garrett, of Salt Lake City, issued the first fascicle of twenty-five numbers of Utah fungi under the title of 'Fungi Utahenses.' This fascicle is devoted to the Uredineæ, and it is the author's intention to follow this plan in succeeding issues, so that each will contain specimens belonging at least to closely related groups. Mr. Garrett has modeled his collection upon the pattern so well set by Professor Kellerman in his 'Ohio Fungi.' The specimens are carefully selected, and neatly put up. Each species is accompanied by a reprint of the original description. This distribution should have a large sale, especially among eastern botanists.

PHOTOGRAPHS OF VEGETATION.

Two years ago the first fascicle of an important work was issued by Gustav Fischer, of Jena, under the simple title of 'Vegetationsbilder.' Since then nine fascicles have appeared, and it is now possible to judge quite accurately in regard to its usefulness. The work is edited by Professor Doctor G. Karsten, of the University of Bonn, and Professor Doctor Schenck, of the Technical High School of Darmstadt. Each fascicle includes six large reproductions (16 by 21 cm.) of photographs of vegetation, each accompanied by a page or two of explanatory text. The first fascicle is devoted to the vegetation of southern Brazil and is the work of Dr. Schenck. The second fascicle, by Dr. Karsten, illustrates the vegetation of the Malayan Archipelago; the third is again by Dr. Schenck, who takes up some economic plants of the tropics, as *Thea sinensis, Theobroma cacao, Coffea arabica,* etc. Fascicle 4, devoted to tropical and subtropical Mexican vegetation, is edited by Dr. Karsten, while the next, relating to southwest Africa, is from the hand of Dr. Schenck. In the latter the most remarkable is Plate 25, showing a desert, with several plants of *Welwitschia mirabilis* in the *foreground.* The sixth fascicle, by Dr. Karsten, contains photographs of six species of monocotyledonous trees, including one plate of a giant bamboo clump (*Dendrocalamus giganteus*) forty meters high, on the island of Ceylon. Six views of Brazilian strand vegetation, by Dr. Schenck, make up the seventh fascicle, and another half dozen devoted to Mexican cactuses and agaves (by Dr. Karsten and Dr. Stahl) make up the eighth fascicle which closes Series I. of the publication. The second series opens with a fascicle by E. Ule, on Amazonian epiphytes, in which the illustrations and text maintain the high degree of excellence of the first series.

It is to be hoped that this most useful publication will be continued until the vegetation of the more important parts of the world have been depicted and described. It certainly fills a place in the botanist's library that has not hitherto been occupied.

Charles E. Bessey.
The University of Nebraska.

SCIENTIFIC NOTES AND NEWS.

We regret to learn that Lord Kelvin is ill after a surgical operation.

A statue of Professor Conrad Röntgen has been placed on one of the bridges in Berlin.

Professor Wilhelm Ostwald, the eminent physical chemist of Leipzig, will again this year take part in the work of the summer school of the University of California.

Dr. Frank Schlesinger has been elected director of the New Allegheny Observatory, and assumed the duties of the office on April 1. The observatory has an endowment fund, and a regular income from the time service, besides owning a large and valuable property in the City of Allegheny, which will be-

come a source of income in the near future. Work has not been suspended on account of lack of funds and much has been accomplished toward the instrumental equipment during the year past. The Keeler Memorial Telescope of 30 inch aperture is now ready to be set up, and the large (Porter) spectroheliograph is almost completed. The 30 inch objective is well under way, and other instruments will be installed during the year under the directorate of Dr. Schlesinger.

OWING to the appointment of Dr. C. H. Wind, director of the Dutch Meteorological Institute, to a professorship in the University of Utrecht, Dr. E. Van Everdingen has been made acting director of the institute.

THE Academy of Sciences of St. Petersburg has awarded its Lomonsoff prize to Professor N. A. Menschutkin for his researches in theoretical chemistry and its Ivanoff prize to Professor P. N. Lebedeff, of Moscow, for his work on light pressure.

THE French Société d'encouragement pour l'industrie nationale has awarded the Lavoisier medal to M. Héroult in recognition of his electrometallurgical researches.

THE Leopoldo-Carolinische Akademie of Halle has awarded the gold Cothenius medal to Professor E. von Leyden, of Berlin, for his services to scientific medicine.

ABERDEEN UNIVERSITY will confer the Doctorate of Laws on Mr. J. T. Merz, author of 'European Thought.'

THE thirteenth 'James Forrest' lecture of the British Institution of Civil Engineers will be delivered by Colonel R. E. B. Crompton on April 10, on 'Unsolved Problems in Electrical Engineering.'

PROFESSOR RICHARD ANDREE, the ethnologist and geographer, long editor of *Globus*, has celebrated his seventieth birthday, in Munich.

PROFESSOR FRANCIS E. LLOYD, Teachers College, Columbia University, has received a grant of $500 from the Carnegie Institution to aid him in continuing his studies on stomatal action and transpiration in desert plants. He will spend three months at the Desert Botanical Laboratory, Tucson, Ariz., for this purpose.

DR. ALFRED ERNST, associate professor of botany at Zurich, has been given the Swiss subvention of $1,000 for botanical studies at Buitenzorg.

A MEMORIAL tablet for the late Dr. N. S. Davis was presented by the senior medical class of the Northwestern University Medical School, on March 24, in Davis Hall.

WE regret to record the death of Dr. Emmerich Meissl, section chief in the Department of Agriculture at Vienna; of Dr. Richard Sadebach, director of the Botanical Museum at Hamburg, and of Father Timoteo Bertelli, the Italian geophysicist.

THERE will be on May 3 and 4 a civil service examination for topographic draftsman in the Panama Canal work, at salaries of from $900 to $1,500.

To secure promptness in the delivery of the reference cards issued by the Concilium Bibliographicum of Zurich, Dr. H. H. Field has placed a stock series in the American Museum of Natural History. Any worker in biology, who does not receive the cards directly from Zurich, by writing to the museum will receive by return mail cards bearing upon such minor subjects as he may indicate, at the same terms as though the delivery were made from Zurich, and with an obvious saving of considerable time. Communications bearing upon this matter should be addressed to the American Museum of Natural History, Department of Books and Publications.

OXFORD convocation has passed a decree authorizing the contribution by annual instalments of a sum not exceeding £1,000 towards the expense of printing that portion of the British Section of the International Astrographic Catalogue, which has been carried out at the University Observatory, the Treasury, on the representations of the Royal Society, contributing the other moiety.

MR. ALEXANDER FRY has bequeathed his entomological collection to the British Museum of Natural History.

Nature states that the collection of birds' eggs possessed by the British (Natural History) Museum has been largely augmented by

the gift of the splendid series brought together by Mr. W. Radcliff Saunders, of High Bank, Tonbridge. This collection comprises close on ten thousand specimens of the eggs of Palearctic species, together with one hundred and sixty-five nests.

The Crosby Brown collection of musical instruments, in the Metropolitan Museum of New York, to which we have several times referred in recent years, continues to grow. The recently published new edition of the catalogue of European instruments is increased over the previous edition by more than fifty pages and three plates. The total number of instruments is given as about 3,200, besides hundreds of parts illustrating elements and details of construction.

Reuter's Agency reports some details of an expedition which went to British New Guinea in September, 1903, and has lately returned to England. The expedition was organized by Major W. Cooke-Daniels, an American traveler, and it also included Dr. C. G. Seligmann, Dr. W. M. Strong and Mr. A. H. Dunning. The objects were primarily ethnographical, but studies were also made in other branches of science, and a number of general pathological observations were made.

The annual dinner of the British Institution of civil engineers took place on March 22, with Sir Guilford Molesworth in the chair.

The sixth International Congress of Applied Chemistry will be held at Rome next year, probably during the week following Easter.

At the last International Ornithological Congress, which was held in Paris in 1900, it was decided that the next session of the congress should take place in London after an interval of five years, and, as we have already announced, arrangements have been made for holding the fourth congress from June 12 to 17, under the presidency of Dr. Bowdler Sharpe, of the British Museum. The London *Times* states that it has been decided by the organizing committee to divide the congress into general meetings and meetings of sections, of which there will be five, as follows: (1) Systematic Ornithology; Geographical Distribution, Anatomy and Paleontology; (2) Migration; (3) Biology, Nidification, Oology; (4) Economic Ornithology and Bird Protection; and (5) Aviculture. It is expected that many interesting papers on these various subjects will be forthcoming. The social side of the program is not being neglected. Thus it is proposed to devote one day to an excursion to Tring to inspect the collection of birds belonging to Mr. Walter Rothschild. On this occasion there will be lectures, and the members will be the guests of the owner of the Tring Museum. On the 16th of June the congress will be received by the Lord Mayor of London at the Mansion-house, and the visitors will be shown over the Guildhall by Alderman Treloar. At the close of the proceedings in London, on the invitation of the Duke of Bedford, an excursion will be made to Woburn to view the collection of live animals in Woburn Park, and the following day will be spent at Cambridge, where Professor Newton will welcome the members at Magdalene College. Finally, a journey has been planned to Flamborough Head, in Yorkshire, of special interest to continental ornithologists, as affording them an opportunity of seeing the breeding place of so many sea birds, while the season of the year may also allow of their watching the operations of the collectors of the eggs of the guillemats.

An international exhibit of hygiene will, by request of the Italian general health department, be added to the exhibition at Milan, 1906. Many countries will assist in the display, which will have for its object the presentation in a practical manner to the public of the advances made in the field of sanitation during recent years.

Medical journals state that the Hamburg-American line has placed the *Hamburg* at the disposal of the German physicians and their families who wish to attend the International Medical Congress at Lisbon next year. The vessel will be moored at some convenient wharf and will serve as a hotel for the passengers during the congress. After a brief trip to Gibraltar and Madeira the vessel will return to Hamburg. Messrs. Cook & Son have offered to charter a passenger ship for the English

members of the congress, to serve as a floating hotel.

WE learn from *The British Medical Journal* that the proceedings of the Pan-American Medical Congress, held at Panama in January, closed with a meeting of the executive committee at which the following resolutions, proposed by Guatemala and seconded by Peru, were carried: (1) That the next Pan-American Medical Congress arrange (*a*) for an international American pharmacopoeia; (*b*) for an international code of sanitation; (*c*) for an international code on temperance; and (*d*) as a sequel to the above, for the establishment of sanatoriums for the treatment of alcoholism; (*e*) for the formation of lectureships on medicine in the required studies of jurisprudence. (2) That there be formed at the next congress a section on tropical diseases. (3) That there be created Red Cross Branches, both civil and military. (4) Finally, it was resolved that encouragement be given to those engaged in the campaign against tuberculosis.

Nature states that after an interval of two years the fifth conference of West Indian agriculturists was held at Port-of-Spain, Trinidad, from January 4 to 13. It was attended by official, scientific, commercial and practical representatives from all parts. In his presidential address, Sir Daniel Morris gave an interesting survey of the great economic change which is in progress. Taken in the aggregate, sugar cultivation must still be regarded as the backbone of the colonial industries, but in some of the islands it has already become of comparatively little or no importance. Trinidad is now a cacao-producing island, its exports of this commodity having risen to the value of a million sterling per annum. Grenada's cacao exports are valued at £250,000 and Jamaica's at £80,000. Cotton growing, too, has been successfully reestablished in several islands, and remunerative prices for the raw cotton are being obtained from Lancashire merchants. The exportations of fruit far exceed in value those of the staple industry. The development of the tobacco, rubber, sisal hemp, fish-curing, and other industries also came under review, and Sir Daniel dwelt upon the importance of agricultural shows and on the provision made by his department for teaching elementary science and the principles of agriculture in the various colleges and elementary schools. Numerous papers were read and discussed, Professor d'Albuquerque, Dr. Watts, Professor Harrison and others supplying valuable information relating to sugar; Mr. Hart, Mr. de Gannes and others, on cacao; Mr. Bovel, Mr. Sands and others, on cotton; and so on. For practical purposes visits were paid to several cacao and sugar estates. Owing to its more than usually representative character the conference is declared to have been the most successful of the series.

UNIVERSITY AND EDUCATIONAL NEWS.

THE New York *Times* states that Mr. Carnegie since 1900 has given $2,000,000 for libraries and $4,500,000 for other buildings and endowments for colleges. The following is a list of institutions to which the money has been given:

Pennsylvania State College	$150,000
Upper Iowa University	25,000
Oklahoma University	30,000
Iowa College	50,000
Tuskegee Institute	20,000
Beloit College	50,000
Cornell College	50,000
Mt. Holyoke College	50,000
Yankton College	15,000
Talladega College	15,000
N. C. State Normal and Industrial	18,868
Lebanon Valley College	20,000
Wilberforce University	15,000
Bucknell University	30,000
Berea College	30,000
Agricultural and Mech. College	12,000
Winthrop Normal and Ind. College	30,000
Washburn College	40,000
Converse College	10,000
Benedict College	6,000
Park College	15,000
Atlanta University	25,000
Tufts College	100,000
Lawrence University	50,000
Marietta College	40,000
Oberlin College	125,000
Fairmount College	40,000
University of Maine	50,000
Bethany College	20,000

North Dakota Agricultural College.	15,000
Simpson College	10,000
Carson & Newman College	10,000
Central University of Kentucky	30,000
Earlham College	30,000
Fessenden Academy	5,000
Fisk University	20,000
Furman University	15,000
State Normal School	10,000
Heidelberg University	25,000
Juniata College	15,000
Livingstone College	12,500
Pomona College	40,000
Syracuse University	150,000
University of Mississippi	25,000
University of Tennessee	40,000
Washington and Lee University	50,000
De Pauw University	50,000
University of North Carolina	50,000
Alabama Polytechnic Institute	30,000
Drake University	50,000
St. John's College	16,700

During the past week a gift of $100,000 to Rochester University for the construction of a scientific building and $50,000 to Norwich University, Vermont, half for a library and half for an engineering department, have been announced.

It is announced that a gift of $250,000 has been made to Northwestern University by Milton H. Wilson, a resident of Evanston, and one of the trustees of the institution.

The London *Times* states that further papers have been published by the government of India in respect to the late Mr. Jamsetjee N. Tata's offer of an endowment in the shape of properties valued at £200,000 for the creation of an Institute of Indian Research at Bangalore. Certain conditions in respect to government assistance were attached to the offer, which was first made six years ago, and these have been the subject of prolonged discussion and correspondence between the government, Mr. Tata during his lifetime, and his representatives. The papers now published show that the difficulties in the way of a settlement have been removed. Guarantees have been offered by the representatives of the donor to secure the full income estimated from the endowment properties, and the management of the latter is vested in a board whose chairman is to be an officer selected by the Bombay government. In addition to making a grant of 2½ lakhs of rupees (£16,666) towards the construction of the necessary buildings and provision of scientific apparatus, the government will make an annual grant to the institute of half the local assets up to a limit of 1½ lakhs of rupees, provided that the institute is conducted on lines approved generally by the government.

The Barney Memorial Science Hall of Denison University was completely destroyed by fire at two o'clock A.M., March 30. The building was erected in 1894 at a cost of $45,000, and the value of the equipment within it was also about $45,000. The department of chemistry, in which the fire started, suffered total loss. The department of physics, which had installed several thousand dollars worth of new apparatus this season, lost most of its equipment, including the greater part of the manuscripts and instruments of the researches of Professor C. W. Chamberlain. The equipment of the department of geology, including about $7,000 worth added this year, was nearly all saved. The departments of zoology and botany saved about half of their equipment. There is about $40,000 insurance on the building and contents, and the hall will be rebuilt at once. The most serious loss is the scientific library which has been accumulated during the past twenty years, chiefly through exchange with Denison publications and which was one of the richest collections of society transactions and similar serials in the middle west. In making good, so far as possible, this severe loss the generous cooperation of authors and learned societies is requested. Authors' separates, duplicates, or society publications, if sent to the permanent secretary of the Denison Scientific Association, Granville, Ohio, will be gratefully accepted as a nucleus of a new library.

Rev. Frederick W. Hamilton, D.D., pastor of the First Universalist Church of Roxbury, has been elected temporary president of Tufts College, to fill the vacancy caused by the death of President Capen.

Mr. R. P. Gregory, M.A., of St. John's College, has been appointed senior demonstrator in botany at Cambridge.